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Intonation in Musical Performance

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THOSE who enjoy listening to music agree that their pleasure is marred if the music is out of tune. The performer, curiously enough, is often unaware of lapses in intonation[1] which are obvious to the listener; but that is usually because he is inattentive to what he is doing. His own pleasure chiefly comes from the surmounting of technical difficulties and the expression of strong emotion. If he is not highly talented and thoroughly trained, he may not listen critically to his own performance.

The hotly debated question of how much out-of-tuneness can be tolerated by the listener is so bound up with his natural endowment and musical training that it can never be wholly settled. However, the scientist can make useful and interesting contributions to the discussion by furnishing answers to such objective questions as these: What standards of musical intonation have been set up? Is the normal ear sensitive enough to distinguish between these standards? Is there a one-to-one correspondence between pitch and frequency? How much control has the musician over his intonation? What criterions does he employ in seeking to improve it? What experimental procedures will give data on intonation? Are there inherent discrepancies between the various groups of instruments? Is there any scientific evidence for or against the opinion that unaccompanied strings or voices make better music than that produced by other means? Can there be one standard of intonation, applicable alike to all kinds of music?

There are three standards of present-day musical intonation: the just scale, the Pythagorean scale and the equally tempered scale.[2] These may be compared with the aid of Tables I(a) and I(b).

TABLE I(a). Scale of *C Major*.* Frequencies in cycles per second.

Just	261.6	294.3	327.0	348.8	392.4	436.1	490.6
Pythagorean	261.6	294.3	331.1	348.8	392.4	441.5	496.7
Equally tempered	261.6	293.7	329.6	349.2	392.0	440.0	493.9

* Minor scales are discussed in Lloyd, *Music and sound* (Oxford University Press, 1937), p. 14.

[1] The word *intonation* is here used in a special sense, familiar to musicians, meaning *conformity to some standard of pitch*. See *Webster's new international dictionary* (Unabr. ed., Merriam, 1936), p. 1302; Grove, *Dictionary of music and musicians* (Macmillan, 1911), p. 488.

[2] Only Western music is considered here. A discussion of scales used by Eastern nations is given in Helmholtz, *Sensations of tone*, tr. by Ellis (Longmans Green, 1912), p. 514.

TABLE I(b). Major Scale. Ratios in logarithmic cents.†

Just	0	203.9	386.3	498.0	702.0	884.4	1088.3
Pythagorean	0	203.9	407.8	498.0	702.0	905.9	1109.8
Equally tempered	0	200.0	400.0	500.0	700.0	900.0	1100.0

† The number of logarithmic cents L in an interval expressed by the ratio n_1/n_2 is given by the equation $L = 1200 \log (n_1/n_2)/\log 2$.

Although the just scale makes an immediate appeal to physicists by reason of its kinship to the series of natural harmonics, there is little or no scientific evidence that it is the goal toward which all musicians are striving. As a matter of fact, few have heard it, and fewer have practised it. Here and there one may find an experimental organ tuned to the just scale; the musical usefulness of such an instrument is often limited by small size and inability to play in all keys.[3] Some musicians deny the desirability of the just scale.[4] With questions of this kind the physicist is not primarily concerned. What he may properly inquire is whether the musician can distinguish aurally between the different kinds of intonation, and whether he actually does favor one more than the others in performance.

The physiology of pitch discrimination is ably presented in a recent work of reference.[5] As might be expected, it is shown experimentally that the ear has a region of maximum sensitivity to changes in pitch. This coincides with the region of maximum sensitivity to sound energy, extending from 2000 to 4000 cycle/sec. Talented and practiced observers can detect in this region a pitch-difference of about 1/50 of a semitone between two pure tones of equal intensity, presented successively under laboratory conditions and without musical accompaniment. This is equal to the smallest discrepancy between just intonation and equal temperament. It should be noted, however, that if the observer is tested with apparatus producing tones which can be altered in either frequency or intensity, and is not told which kind of change is being made, he cannot tell a frequency-change from an intensity-change unless it is much larger than the afore-cited minimum, perhaps as large as 1/10 of a semitone.[6] Even this interval is smaller than some of the conspicuous discrepancies in Table I, the largest of which amount to about 1/6 of a semitone.

With the complex tones of music, discrimination is easier.[7] In the concert room the trained ear is fully capable of determining whether an isolated interval or a musical phrase is in just intonation or in equal temperament.

It is a well-established fact that the subjective attribute called *pitch* is not solely dependent upon frequency; loudness and quality enter into it.[5,7] Conspicuous deviations[8] ascribable to this cause occasionally take place in musical performance. For example, Wagner's overture to *Rienzi* begins with a sustained and unaccompanied trumpet note in the middle register, played thus: *pp-cresc.-ff-dim.-pp*. To the performer the pitch appears to flatten as the note gets louder; hence he may try to "lip it up," sharpening it correctly for himself but excessively for the listener who is not so near the trumpet. Woodwind players have similar troubles. They know that in the modern orchestra their solo entries, particularly in the low register, are all too likely to be buried; so they yield to the temptation to overblow. Just what happens to the pitch is complicated by the natural behavior of the instruments themselves—some go sharp and some go flat when overblown; but the essential fact for this discussion is that the player does not always hear the same pitch as the listener. Hence many a fruitless wrangle between player and conductor at rehearsal! No doubt vocalists often find it difficult to make unaccompanied entries exactly on pitch for the same reason. Perhaps also the "stretching of octaves" practiced by most piano tuners and declared to be demanded by their clients[9] is thus accounted for;

[3] Jones, J. Acous. Soc. Am. **4**, 3 (1932); Helmholtz, reference 2, pp. 316, 470–483; Williamson, J. Acous. Soc. Am. **11**, 216 (1939).

[4] Much music is too chromatic to be adapted to the just scale. The author believes that music which is simple enough in harmonic structure to be performed in just intonation sounds best when it is so performed. See Lloyd, J. Acous. Soc. Am. **11**, 440 (1940).

[5] Stevens and Davis, *Hearing, its psychology and physiology* (Wiley, 1938), chap. 3.

[6] Information kindly supplied by Dr. Max Schoen from unpublished data.

[7] Fletcher, J. Acous. Soc. Am. **6**, 59 (1934); Lewis and Cowan, J. Acous. Soc. Am. **8**, 20 (1936).

[8] The pitch-deviations referred to in this paragraph may amount to as much as half a semitone.

[9] Railsback, J. Acous. Soc. Am. **9**, 274 (1938); **10**, 86 (1938).

yet organ tuners apparently do not find it necessary.[10]

The singer is the only musician whose intonation is entirely unfettered by the limitations of his instrument.[11] At the opposite extreme is the player of a keyboard instrument, who accepts the tuning as he finds it. Many gradations between these limits are found. For example, the violinist pre-tunes his open strings but adjusts all the other notes at his pleasure during performance. The harpist, on the other hand, can do nothing to improve his intonation while playing; but he does his own tuning, presumably in the tempered scale,[12] and corrects errors as soon as he is free to do so. Woodwinds are supposedly built in the tempered scale; but most of them have notes that are out of tune. Competent musicians learn to "favor" these, usually at some sacrifice of tone quality. Very little control is possible over notes in the lowest octave of any reed instrument. Brasses are built neither in just intonation nor in equal temperament. Their open tones are in the just scale, at least theoretically;[13] but their valves supply intermediate steps that must compromise between conflicting requirements.[14] Players on the slide trombone and French horn can adjust the pitch of each note at will, the former with the slide, the latter with the hand in the bell; the rest of the brasses must be content with what can be managed with the lips alone.

The tempered scale may be approximated quite closely by the routine methods of tuning a keyboard instrument.[15] These methods are primarily physical, though the tuner may test his work by noting the musical effect, and may then vitiate it by favoring some keys at the expense of others! Among the few instruments that permanently retain the relative pitch of their original tuning are the glockenspiel, the celesta, the xylophone and the Hammond organ; the absolute pitch of the first three is affected by temperature or humidity, and that of the last by variations in the frequency of the electric power supply.

Recent developments have made it possible to test the intonation of a musician when he produces only one tone at a time. Work of this kind has been done in America with the Seashore tonoscope and the Conn chromatic stroboscope, and in Japan with an electronic pitch recorder.[16] In three of these investigations phonograph recordings of soprano voices were used. Deviations of almost half a semitone were reported. In this connection it must be remembered that many people who were born with good voices were not born with good ears. Moreover, data obtained from phonograph records must be interpreted with caution. One can achieve a high degree of apparent reliability by repeating readings many times; but the deviations found may not all be due to the original performance. Only the most modern and expensive recording and reproducing equipment will afford freedom from noticeable "wows," that is, cyclic variations in frequency due to decentering of the record or varying angular velocity of the turntable.

Similar tests applied to unaccompanied violin playing appear to indicate a tendency to approximate the Pythagorean scale.[17] This may be

[10] Audsley, *The art of organ building* (Dodd, Mead, 1905), Vol. 2, p. 647.

[11] Even the trombonist must painstakingly learn to correct the errors inherent in his instrument. See reference 13.

[12] Curiously enough, none of the many authorities consulted states whether the standard double-action concert harp is tuned to the tempered scale or otherwise. One instruction book advises the beginner to tune to the piano, note by note. This of course would set the harp in the tempered scale, thus permitting its use in all keys without further adjustment except for the pedals. The same book suggests, however, that a harpist who is to play in a given key will do well to refine the tuning in this key. This looks like a hint at just intonation. One is tempted to infer that the harpist should develop and use his own judgment! At that, deficiencies in intonation are probably not conspicuously noticeable in an instrument whose tones die away so rapidly. The author has found them much easier to detect in the organ than elsewhere.

[13] This is the obvious prediction from simple theory; but actual instruments often have one or more open tones that are manifestly out of tune with the rest. See Young, J. Acous. Soc. Am. **13**, 333 (1942).

[14] Mahan, quoted in Clappe, *The wind band and its instruments* (Holt, 1911), chap. 4; Redfield, J. Acous. Soc. Am. **3**, 292 (1931). Two British firms have produced brass instruments in which this difficulty is ingeniously overcome; but they remain almost unknown in this country.

[15] Harker, J. Acous. Soc. Am. **8**, 243 (1937); White, J. Acous. Soc. Am. **9**, 47 (1937).

[16] Schoen, Psych. Monogr. **31**, 230–259 (1922); Schoen, *The psychology of music* (Ronald, 1940), chap. 10; Railsback, J. Acous. Soc. Am. **9**, 37 (1937); Young and Loomis, J. Acous. Soc. Am. **9**, 79 (1937); **10**, 112 (1938); Obata and Kobayashi, J. Acous. Soc. Am. **9**, 156 (1937); **10**, 147 (1938); **12**, 188 (1940).

[17] Greene, J. Acous. Soc. Am. **9**, 43 (1937), and Seashore, *Psychology of music* (McGraw-Hill, 1938), p. 218. The musical selection for this research was in the key of *C*.

traceable to the influence of the perfect fifths to which the strings are tuned. It would be more significant if it could be demonstrated when the music is in remote keys such as $D\flat$ and $G\flat$. In these keys the open strings would have little influence.

In solos accompanied by a keyboard instrument, or in concerted performances by small groups, musicians try to arrive at a mutual agreement. One often hears it said that they are seeking a smooth effect. This suggests that they prefer concords which are "beatless"; that is, in just intonation. Tests of isolated concords support this conclusion. Many competent observers incline to the belief that the foremost string and vocal ensembles tend to perform in just intonation; but the complicated and difficult task of testing their music quantitatively has apparently not been attempted. So far as the string choir is concerned, the succession of perfect fifths to which they are tuned precludes the use of just intonation in those keys where all five open tones appear.[18] A well-drilled woodwind ensemble can come pretty close to equally tempered intonation. Now and then in orchestral performances one hears a somewhat startling effect when the woodwind echoes in equal temperament a chord played by strings or brasses in just intonation! The celebrated all-brass bands of the North of England undoubtedly aim at just intonation, and partially achieve it. They play generally in flat keys, in which many justly intoned concords are possible. The trombone choirs of the Moravian churches in Pennsylvania and elsewhere can play in just intonation in every key, because of the unlimited facilities for adjustment which are inherent in the slide mechanism.

Obviously, then, no single standard of intonation can be used for all musical performance. In the symphony orchestra and concert band, because of the differences between the groups of participating instruments, there must be a shifting, a pulling and hauling, to which quite likely part of the charm of the large ensemble may be ascribed. Some unwanted differences are caused by drifts in pitch due to the warming-up of the instruments, not all of which are affected alike by temperature changes.[19] An extreme case is encountered when a marching band attempts to use the same glockenspiel in midsummer and midwinter. If the glockenspiel is in tune with the band in summer, it may be as much as half a semitone sharp in winter.[20] The controlling variable in the case of the wind instruments is the velocity of sound in the air column, which has a positive temperature coefficient; for the glockenspiel it is Young's modulus, which has a negative temperature coefficient.

Having looked at some possible answers to the detailed questions posed earlier in this paper, we may appropriately consider the larger problem of tuning a major force of instrumentalists. Ordinarily a tuning bar sounds the *A*. A recent alternative is the electric tuning fork.[21] The time-honored custom of allowing the oboe to assume this office is less satisfactory. If a keyboard instrument is included, as in a piano concerto, it must sound the *A*. Unless it has been previously tuned to the recognized standard—440 cycle/sec.—at the prevailing concert-room temperature, serious difficulties may arise with the woodwinds, none of which can be tuned much below normal with success.[22] The reason may be seen by making a detailed comparison of clarinets built in $B\flat$ and in *A*. It will be seen that every longitudinal dimension of the $B\flat$ instrument is increased in the *A*. How, then, can a clarinet be tuned appreciably lower? It is true that there are two or three joints that can be drawn a little; but this expedient will not increase the separation of the finger holes on any given portion of the instrument. The thankless and hampering task of "favoring" most of the notes in the scale will be thrust upon the hapless player. Brasses, on the other hand, can be tuned as much as a semitone lower without detriment, since their valve-slides may be drawn proportionately.

After tuning to the *A*, every well-drilled orchestra player proceeds to test and adjust certain other notes on his instrument until they

[18] In *G major*, the *E*'s are too sharp; in *C major*, the *A*'s and *E*'s; in *F major*, the *D*'s, *A*'s and *E*'s.

[19] Barton, *A textbook on sound* (Macmillan, 1926), p. 467.

[20] Information kindly supplied by Captain F. E. Resta, Bandmaster, United States Military Academy.

[21] Eisenbeis and Williamson, U. S. Patent 2,152,177 (Mar. 28, 1939).

[22] No wind instrument can be tuned *above* normal, because no provision is made for shortening the fundamental length.

"conform"; but to what standard? For best results, this should depend on the character of the ensemble. If all the instruments are strings, they may be tuned as usual to perfect fifths; if the only other instruments are brasses, the same procedure is acceptable, since the brasses can approximate to the prevailing intonation, whatever that is; but if woodwinds are added, the strings might better be tuned to tempered fifths; and if a piano or a pipe organ is included, tempered tuning of the strings is obligatory. This, it must be admitted, is a counsel of perfection; but there is nothing impossible about it; if the keyboard instrument has been well tuned to the tempered scale, it can sound five tuning notes in succession, to which each string may be carefully tuned. The wind instruments will then adjust themselves, as is their wont, to the local "atmosphere," which in this instance will be tempered intonation; and the general result will be notably better than the haphazard effect usually heard.

The physicist who has not paid much attention to the minutiae of musical performance may well wonder why all this to-do about intonation. He knows that the human ear is remarkably adept at "hearing what it wants to hear," and he feels that discrepancies of pitch are minor shortcomings which can be glossed over by all but the hypersensitive. There is much truth in this point of view; most music would be unpleasant if not intolerable[23] were it not for these auditory adjustments; but the smaller such adjustments need to be, the better the music is. True, the ordinary listener may not know what is wrong with music that is out of tune; but he will dislike it nevertheless. Although other characteristics contribute to the eminence of the world's greatest singers, it is significant that the best of them sing most nearly in tune.[24] Music, like a three-legged stool, stands on tune, time and tone; with one impaired, it falls to the ground.

[23] An extreme instance is cited by Seashore, reference 17, p. 4.

[24] See reference 16. Seashore, reference 17, chap. 20, discusses the esthetic importance of artistic pitch-deviations.

THE ideology of science and traditional ideology are constantly at war with each other, sometimes in the same individual, but it is certain that the ideology of science is winning the war. The difference between the two is that although both deal with the here-now, traditional ideology always looks backward for its standards, whereas science never looks backward for standards. Science tests everything in the here-now and keeps its face turned always toward the future. Science is like perpetual youth; life is always before it. For six thousand years, at least, civilizations have all been based upon a traditional ideology and have taught that civilization must be so based. Only since the great war have people begun to realize the great change science is causing. Without conscious intention on their part, the work of scientists is slowly killing traditional ideology and substituting a non-traditional, experimental ideology. Whether we like it or not, that is what is happening.—A. A. MERRILL, *J. Frank. Inst.* **223**, 511 (1937).

Early Principles in the Calibration of Thermometers

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HALF a century after the invention of the telescope and the thermometer, the attitude of science toward these instruments presented a striking contrast. Whereas the one had developed speedily into an indispensable tool of astronomy, the other was at that time still regarded as a novelty of doubtful significance. This situation is probably to be explained by the fact that, as the very names of the instruments imply, the one served at first as an aid in qualitative observation, whereas the usefulness of the other was reserved more specifically for quantitative investigation. For this reason, histories of science justly devote much attention to the early calibration of thermometers; but, in this connection, an emphasis which traditionally has been placed upon the earliest suggestions of the use of *two* fixed points[1] has obscured the historical significance, both in practice and in theory, of an alternative thermometric principle based upon only *one* fixed point.

The two-point calibration of thermometers was adumbrated in the Accademia del Cimento (1657–1667) when the scales of the famous Florentine instruments were constructed by dividing into an arbitrary number of equal parts—such as fifty, or one or more hundreds—the interval between the marks indicating the greatest summer heat and the most severe winter cold.[2] However, these extreme temperatures could not be determined with any degree of precision, and hence no standard thermometric system was established by their use. Further progress in this direction was made by Fabri, a theologian, physicist and astronomer connected with the Florentine Academy, who often is hailed as the first to recognize the significance of two fixed points in determining a thermometric system. In 1669 he used snow for his lower extreme, but he retained the indefinite upper limit which had been adopted by the Florentines. In 1688 Dalencé suggested changing the upper fixed point to the melting point of butter, thus affording much greater precision and making possible truly comparable readings. Renaldini in 1693 improved on this work by proposing a scale of 12 divisions between the melting point of ice and the boiling point of water. A half century later these two points became, through the work of Celsius, the basis for the centigrade system.[3]

Newton, Roemer and others also saw the desirability of employing two fixed points.[4] In 1702 Roemer adopted for the lower fiducial point the temperature of a mixture of ice and salt as 0°, and for the upper limit the boiling point of water as 60°. On this sexagesimal system the freezing point of water was about $7\frac{1}{2}°$ or 8° and normal body temperature was $22\frac{1}{2}°$. These latter two intermediate points of Roemer were adopted at about the same time by Newton as the primary upper and lower limits of an interval divided into 12 equal parts.

Through Roemer's work Fahrenheit was led to accept the temperature of a mixture of salt and ice as his lower fixed point and body temperature as the upper. The freezing point of water was retained also as a subsidiary calibration point. However, in order to facilitate readings Fahren-

[1] See, for example, H. Crew, *The rise of modern physics* (1928), p. 194; A. Wolf, *A history of science, technology, and philosophy in the 16th and 17th centuries* (1935), pp. 88–89; Wolf, *A history of science, technology, and philosophy in the eighteenth century* (1939), p. 307; H. C. Bolton, *Evolution of the thermometer* 1592–1743 (1900), pp. 43, 46, 50 f, 56 f; K. Meyer, *Die Entwicklung des Temperaturbegriffs im Laufe der Zeiten* (1913), pp. 61–63; T. Preston, *The theory of heat* (1894), p. 105; F. Burckhardt, *Die Erfindung des Thermometers und seine Gestaltung im XVII Jahrhundert* (1867), pp. 44 f; E. Mach, *Die Principien der Wärmelehre historisch-kritisch Entwickelt* (1896), pp. 9, 11. See also I. B. Cohen, "The first determination of the velocity of light. (Roemer 1676)," Isis **31**, 327–379 (1940), in particular, p. 360; the appearance of this number of *Isis* in America has been delayed by the German invasion of Belgium, but it is to be hoped that it soon will be available.

[2] *Essayes of natural experiments made in the Accademie del Cimento* (tr. by Richard Waller, London, 1684), pp. 2–5, 82, 100. Good accounts of the work of the Florentine Academy appear in M. Ornstein, *The rôle of the scientific societies in the seventeenth century* (1913), pp. 93–109, and in the first volume of Wolf (reference 1), pp. 54–59, 87, 307.

[3] See A. J. von Oettingen, *Abhandlungen über Thermometrie von Fahrenheit, Réaumur, Celsius (1724, 1730–1733, 1742)*, Ostwald's *Klassiker* No. 57 (1894), pp. 117–124.

[4] See Meyer, reference 1, pp. 66 ff; also Cohen, reference 1.

heit multiplied the number of Roemer's degrees by four, and then later changed the upper limit from 90° to 96° to establish the present Fahrenheit scale[5] on which the boiling point incidentally fell at 212°.

The use of two fiducial points thus is seen to have played a large part in establishing standard scales and making the thermometer an instrument of precision. This fact has led to a general impression that two fixed points "are required" or "are an absolute necessity if the degrees of temperature are to be definitely defined."[6] However, there is an alternative principle, far too frequently overlooked, which quite early was suggested and used to make possible precise and comparable readings under similar reproducible circumstances. In the year 1665 this principle was proposed independently by Boyle, Hooke and Huygens, the latter two of whom were on numerous occasions involved in questions of priority. Hooke's calibration of thermometers may have been suggested to him by the experiments of Boyle, for whom he acted as assistant. In *New experiments and observations touching cold* (1665), Boyle deplored the lack of a standard scale, considered the difficulties in establishing one or two fixed points, and measured the thermal expansions of numerous substances. He determined that oil of turpentine in dropping from a moderate temperature to the freezing point of water lost 1/94th of its volume; and that in dropping further to the temperature of a mixture of snow and salt, the oil lost about 1/47th of its volume.[7] Such observations seem to have suggested to him the principle of an objective thermometric system involving only *one* fixed point—the boiling point—the proportionate expansions or contractions of 10,000 parts of water being used to determine the degrees.[8]

The principle of Boyle was enunciated more explicitly by Huygens and Hooke at about the same time. The state of thermometry led Huygens[9] to write on January 2, 1665:

> It would be a good thing to devise a universal and determinate measure of cold and heat; first making the capacity of the bowl to have a definite proportion to that of the stem, and then taking as the starting point the degree of cold at which water begins to freeze, *or else* the degree of heat of boiling water; so that without sending thermometers one should be able to communicate the degrees of heat and cold which have been found in experiments, and to record these for posterity.

In the famous *Micrographia* (1665) Hooke[10] suggested this principle in still more precise detail:

> Then, for graduating the stem, I fix that for the beginning of my division where the surface of the liquor in the stem remains when the ball is placed in common distilled water, that is so cold that it just begins to freeze and shoot into flakes; and that mark I fix at a convenient place of the stem, to make it capable of exhibiting very many degrees of cold, below that which is requisite to freeze water: the rest of my divisions, both above and below this (which I mark with a 0 or nought) I place according to the Degrees of Expansion, or Contraction of the Liquor in proportion to the bulk it had when it indur'd the newly mention'd freezing cold.

Hooke then went on to describe fully the method which he used in calibrating thermometers in accordance with this principle; each mark on the scale corresponded to an expansion or contraction of 1/1000 of the volume of the thermometric fluid at the fixed point.

It is evident that the procedure indicated by Boyle, Hooke and Huygens would serve as well as that suggested later by Fabri, Dalencé, Renaldini and others to make possible a universally comparable measure of temperature. Hooke often has been referred to as "the universal claimant" because of his pretensions as the anticipator of the discoveries of others, but here is one instance where a significant contribution in which he shared has not received the recognition it deserves. Hooke[11] boasted that "A thermometer, thus marked and prepared,

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[5] See von Oettingen, reference 3, pp. 1–18; J. N. Friend, "The origins of Fahrenheit's thermometric scale," Nature **139**, 395–398, 585–586 (1937); Cajori, "Note on the Fahrenheit scale," Isis **4**, 17–22 (1921–1922).

[6] See Crew, Cohen and Meyer, reference 1; also H. S. Allen and R. S. Maxwell, *A text-book of heat* (1939), vol. 1, p. 7.

[7] *The works of the Honourable Robert Boyle* (Ed. 2, London, 1772), vol. 2, pp. 520–521; also pp. 489 ff.

[8] Friend, reference 5.

[9] *Œuvres complètes* (The Hague, 1888–1937), vol. 5, p. 188. The italics in the quotation are mine.

[10] *Micrographia*, comprising vol. 13 of R. T. Gunther, *Early science in Oxford* (Oxford, 1923–1938); see pp. 38–39.

[11] Reference 10.

will be the fittest instrument to make a standard of heat and cold that can be imagined." This over-bold claim was made without adequate recognition of such difficulties as those pointed out later by Deluc and others—the relative expansions of the container and the thermometric fluid, and the aging of the glass envelope. However, inaccuracies on this score will occur also when the thermometer is calibrated on the principle of dividing into equal parts the interval between two fixed points.

The principles both of one and of two fixed points were familiar to scientists of the early eighteenth century. Newton in particular seems to have made use of both procedures.[12] In fact, the two frequently were confused in interpretations of thermometric systems. Boerhaave, Musschenbroek and Wolf incorrectly inferred that Fahrenheit's scale was suggested and established by the fact that 11,124 parts of mercury at the temperature of a mixture of salt and ice expanded by 32 and 212 parts, respectively, on being warmed to the freezing and boiling points of water, and by 600 parts when raised to the boiling point of mercury.[13] This would imply that his thermometric divisions were determined by the principle of Boyle, Hooke and Huygens. Such a view persisted into the nineteenth century when Young[14] interpreted Fahrenheit's degrees as expansions by 1/10,000 the volume of mercury at a single fixed point. Precisely such a thermometric system was indeed popularized in Russia by De l'Isle who chose the boiling point of water as his single fixed point, but the procedure of Fahrenheit was different. It is now known that the Fahrenheit temperatures were based directly on the two fixed points and scale of Roemer.[15] The principle of one fixed point, however, was definitely used in the establishment not only of De l'Isle's system but also of two other thermometric systems which have continued to rival those of Fahrenheit and Celsius in popularity. One of these was developed by Réaumur, who appears not to have known of Fahrenheit's work when, in 1730 and 1731, he published in the *Mémoires* of the Académie des Sciences two long papers on thermometry. In this work he used alcohol as the thermometric substance, rejecting mercury because of its small coefficient of expansion. The freezing point of water was selected as his single fixed point, 0°. His divisions were then determined upon the principle of proportionate expansion suggested by Boyle, Hooke and Huygens, each degree representing an expansion of 1/1000 the volume at the freezing point.[16] Although the principles of this method of calibration were satisfactory, Réaumur experienced obstacles of a practical character. The chief of these was the difficulty of obtaining alcohol of a standard quality. (Fahrenheit's contribution to precise thermometry had consisted largely in his solution of this difficulty through the purification of his thermometric substance, mercury.) In seeking to overcome this objection Réaumur resorted to the principle of two fixed points to determine the purity of his thermometric fluid. He selected alcohol of such quality that in heating it from the ice point to the steam point the expansion should be 80 parts in 1000. Nevertheless, his work had been inspired by the principle of a single fixed point, and, had chemistry been further advanced in Réaumur's day, he might well have dispensed with his second fixed point.

Réaumur's use of the one-point principle has been regarded as a step backward from the two-point method as used by Fahrenheit.[17] Although there can be no doubt that the thermometers of Fahrenheit were superior in accuracy to those of Réaumur, such a judgment with respect to theoretical principles at that time may well be questioned.

Of greater importance by far than Réaumur's

[12] See W. F. Magie, *A source book in physics* (1935), pp. 125–128.

[13] Boerhaave, *Elements of chemistry* (tr. by T. Dallowe, London, 1735), vol. 1, p. 106; P. van Musschenbroek, *The elements of natural philosophy* (tr. by Colson, London, 1744), vol. 2, pp. 12–13; J. Black, *Lectures on the elements of chemistry* (1st American ed., Philadelphia, 1806–1807), vol. 1, pp. 63–64.

[14] T. Young, *A course of lectures on natural philosophy and the mechanical arts* (London, 1807), vol. 2, p. 386; also Preston, reference 1, p. 109.

[15] Meyer, reference 1; Friend, reference 5.

[16] *Histoire et mémoires de l'académie des sciences* (1730–1731): see vol. 51, *Histoire*, pp. 12–32; vol. 52, *Mémoires*, pp. 645–723; vol. 53, *Histoire*, pp. 7–20; vol. 54, *Mémoires*, pp. 354–418. (References here and elsewhere in this paper are to the Amsterdam edition.) This work will be found also in German translation in von Oettingen, reference 3, pp. 19–116. For a summary in English of Réaumur's methods see Wolf's second volume, reference 1, pp. 308–311.

[17] Meyer, reference 1, p. 85.

practical application of the principle of proportionate expansions was the advance in theory made possible by a corresponding, but earlier, study on the part of Amontons. Boyle, Newton, Halley and Amontons all had been occupied with the measurement of thermal expansions of various substances. Amontons, in particular, corrected barometers for changes of temperature on the basis of an expansion of mercury by 1/115 of its volume in rising from the greatest cold in Paris to the point of greatest heat.[18] A greater significance, however, is to be attached to his analogous observations on air which he made around the turn of the century. Boyle, a generation earlier, had measured the expansion of air with temperature,[19] but his Baconian aversion to hypotheses had prevented him from drawing any general conclusion from his work. It therefore remained for Amontons to discover that at constant volume the pressure of air increased, for equal increments in temperature, by the same fraction of its pressure at some one fixed point. If, for example, in the spring or fall of the year air at the temperature of the atmosphere is heated to the boiling point, the volume remaining constant, the pressure is increased by about one-third, no matter what the original pressure may have been.[20] Amontons saw that this discovery made possible the construction of accurate gas thermometers based on the principle of a single fixed point. His idea was to express the temperature in terms of the proportionate increase or decrease in the pressure of a constant volume of air originally at the temperature of boiling water. Assuming a pressure of 73 units at the boiling point, he determined[21] that greatest summer heat fell at 58 on his scale and the freezing point at about $51\frac{1}{2}$. Moreover, Amontons recognized that this result implied, on the basis of proportionate contractions in the pressure, the existence of an absolute zero—about −240°C from his data—at which air presumably would no longer exert any pressure. He therefore regarded his thermometer as the "natural" measure of absolute heat.

At the time of this work there was no definite recognition of the independent existence of distinctive gases. Amontons therefore had no more opportunity than had Boyle and Mariotte a generation before to generalize his conclusion for gases other than air. Moreover, his results remained relatively unknown—except for a few scientists, such as Lambert, Charles, Saussure, and Volta[22]—for just about a century, after which they were re-enunciated for the expansion of gases in general by Gay-Lussac (and, less precisely, by Dalton).

Gay-Lussac hesitated to accept the point at which gases no longer exert a pressure as an absolute zero for other substances also. However, half a century later Lord Kelvin, from a consideration of Carnot's work on heat engines, developed a scale that is independent of the property of any particular substance. If the zero on this thermodynamic scale is made to agree with the zero of the (ideal) gas scale of Amontons and Gay-Lussac, readings on the two will be identical at all points, thus vindicating Amontons. Although the absolute scale of temperature now is based fundamentally on equal quantities of work rather than on equal increments in volume or pressure of some substance, nevertheless, the inspiration for this scale may be traced historically to the use by Amontons of the one-point principle in thermometry.

In the calibration of thermometers it is sometimes convenient to distinguish between a thermometric *scale*—characterized by the particular substance and property employed—and a thermometric *system*—depending upon the manner in which fixed points and divisions are determined. Thus one might speak of the mercury-in-glass scale or the thermodynamic scale,[23] the degrees of which may be indicated in either the Fahrenheit or the centigrade system. *Degrees Kelvin*, for example, are based on the

[18] *Histoire et mémoires de l'Académie des Sciences* (1704): vol. 8, *Histoire*, pp. 1–10; *Mémoires*, pp. 224–235, 364–373.

[19] *Works*, vol. 2, pp. 601–602.

[20] *Histoire et mémoires de l'Académie des Sciences* (1702): vol. 4, *Histoire*, pp. 1–11, vol. 5, *Mémoires*, pp. 204–231.

[21] *Histoire et mémoires de l'Académie des Sciences* (1703): vol. 6, *Histoire*, pp. 7–12, *Mémoires*, pp. 64–65. See also J. Dewar, "History of cold and the absolute zero," Nature **66**, 467–477 (1902).

[22] See W. J. Lyons, "Early history of the gas laws," Am. J. Phys. (Am. Phys. T.) **6**, 256 (1938); I. Guareschi, "Sulla legge della dilatazione dei gas di Volta," *Archiv für die Geschichte der Naturwissenschaften und der Technik* **5**, 142–154, 209–225 (1913–1915).

[23] For an excellent account of the difficulties involved in reconciling values as given by these two scales see *The physical papers* of H. A. Rowland (1902), pp. 343–468. For accounts of these and other scales see the elaborate volume on *Temperature, its measurement and control in science and industry* (1941), published under the auspices of the American Institute of Physics.

thermodynamic scale and measured in accordance with the centigrade system. It is apparent that the choice of one or two fixed points does not concern the scale but only the system. The degrees in any case may be defined in terms of the proportionate expansion of some thermometric substance. Fahrenheit defined his unit of expansion as 1/96 the total expansion from one natural fixed point to another; Réaumur defined his unit as an expansion by 1/1000 the volume at a single natural fixed point. For any one thermometric substance, the degrees as defined in these two systems are related through a simple linear function. In a sense, then, the two methods are equivalent in that they apply to the same scale, giving rise only to different systems of measurement. In practice, also, there are aspects in which the two resemble each other. On the principle of one natural fixed point, after one or more other points have been chosen on the basis of equal increments in volume, further intermediate divisions can be secured through linear interpolation as in the two-point system. On the other hand, for precision work with mercury-in-glass thermometers based on two natural fixed points, calibration is often effected by breaking the mercury thread into halves or quarters and sliding it along the tube so that, despite irregularities in the bore, degrees will represent equal increments in volume as they would on the one-point principle.

This paper should in no sense be interpreted as a plea to return in experimental procedures to the one-point principle. Boyle, Hooke and Huygens all recognized that the freezing and boiling points of water are at least roughly fixed, but the state of thermometry at the time prevented these men from appreciating fully the nicety of precision which is obtainable through the use of *both* an ice-and-distilled-water bath and a steam jacket. There are experimental difficulties in the method of Boyle, Hooke and Huygens which make the principle of two points experimentally more valuable. Nature has most generously put at the scientist's disposal a great many natural points which serve to mark temperatures with great accuracy. The object of this paper has been twofold: first, to indicate that satisfactory thermometers nevertheless might be established even if nature had been so parsimonious as to afford but a single fixed point; and second, to point out the role which the principle of one fixed point played in the development of thermometry. In the light of this historical development one may say that, of the four thermometric systems now widely used, two—those of Fahrenheit and Celsius—were established by the method of two fixed points, whereas the others—those of Réaumur and Kelvin—were inspired by the principle of a single natural fixed point.

I WEIGH my words well when I assert, that the man who should know the true history of the bit of chalk which every carpenter carries about in his breeches-pocket, though ignorant of all other history, is likely, if he will think his knowledge out to its ultimate results, to have a truer, and therefore a better, conception of this wonderful universe, and of man's relation to it, than the most learned student who is deep-read in the records of humanity and ignorant of those of Nature.—THOMAS HENRY HUXLEY.

The Research Physicist—His Characteristics and Training

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OUR country is now engaged in a great war to test whether the American concept of freedom and the right to pursue happiness shall prevail on the face of this earth. This great new war is essentially a war of production—production of guns; production of transportation, airplanes and ships; production of healthy, trained men; and production of chemicals, metals and materials. To a lesser extent, it is a war of production of new methods, new weapons and new equipment that may tip the balance of military power in our favor. The outcome is primarily in the hands of the production experts. Their competence, training and experience measure the ability of our country to carry the war to a successful conclusion.

A production program of the magnitude contemplated at present will more than exhaust the visible supply of engineers, physicists, chemists, metallurgists and other scientists. Industry and government will certainly need more, many more, highly educated men with special training and exceptional judgment to carry on the war effort.

The universities and colleges, especially those that emphasize technical and scientific work, face a great task, for they must carry forward an educational program under hurried and adverse conditions. This task must be completed with inadequate staffs because the military services have, in one way or another, drafted many of their men. Under such adverse conditions, and because today industry and various emergency organizations are employing the majority of the highly trained men, it is perhaps wise to inquire into those qualities of scientists and engineers that make them most valuable to research organizations carrying forward work for industry and the military services.

The usefulness of any organization is measured by the competence and integrity of its members. A great university with beautiful buildings and superlative equipment is an empty shell without able and stimulating teachers. A research laboratory may be complete in every respect, but it must be carefully staffed with men of ability and imagination if it is to fulfill its function. The administration of any organization whose members to a large extent work independently may well devote a large share of its effort to securing and holding the very best men that can be obtained. Probably the most successful universities and laboratories are those that pursue a wise and farsighted selection policy.

Except for six years of teaching physics in two of the large universities, my experience has been principally in physical research and applied science at the Naval Research Laboratory in Washington. This governmental laboratory is charged by Congress "to increase the safety, reliability and efficiency of the fleet by the application of scientific research and laboratory experimentation to Naval problems." It carries on investigative work in six major divisions—radio, sound, mechanics and electricity, physical optics, chemistry, and metallurgy. The personnel of this laboratory is drawn largely from the ranks of the highly educated and technically trained men and represents, presumably, a cross section of this important group. After years of familiarity with the problems concerned with the selection of these highly trained men and after watching them grow in experience and ability, certain personal convictions have evolved which may be of some interest to those responsible for the training of such men. My remarks are concerned primarily with those characteristics of technically trained men that determine their value to a research organization specializing in the application of scientific principles to military or industrial problems.

I find it convenient to think of a scientist's value to our laboratory as the product of three factors. The first factor is made up of terms that measure the man, his native ability and his capacity for leadership; the second, of terms that measure his education, training and acquired judgment—this product serving as the multiplier of a vector quantity which is proportional to his

ability to give intelligent direction to his energy and experience. I speak of the product of these factors to emphasize the fact that normally a man utterly lacking in any one of these broad qualities is quite valueless to a research organization even though he may be satisfactory in the others.

The first factor, which we may assume measures a man's capacity and native ability, is composed of many terms. Certainly, he must be healthy in mind and in body. He should have a cheerful and cooperative attitude toward his fellow workers. He should not take himself or his troubles too seriously. The man should be an active and interested worker. He should be exceptional in his ability to plan and do things without being told. He should have an active and restless mind, with ability for original thinking. He should be capable of enthusiasm but simultaneously be a lover of truth. The ability to laugh at his own mistakes and weaknesses is usually a sign of a detached interest in human nature which is of great value in certain positions. Finally, he should have some outdoor interest or other recreation in order that his health may be well preserved. Probably no man could completely fulfill all these requirements, but many will achieve a close approximation. Most of the qualities considered can be developed, but to a large extent they are intrinsic. Thus, while the physicist in a key position must work with men having native ability in varying amounts, there is little he can do to extend that ability although he may awaken it by example and by the stimulation of enthusiasm for research.

The second factor, which measures a man's training, experience and judgment, is of greater interest to the educator because he can profoundly modify its value, and he is largely responsible if a man qualifies successfully. It is considered important that a student be early indoctrinated with the basic principles and methods of procedure in science. Although a man of average intelligence will probably acquire this basic understanding in the course of his studies of science, yet it is believed that a definite attempt should be made, early in his training, to provide him with the perspective of the skilled scientist and to acquaint him with those basic methods which normally determine the course of investigation in all research work.

Perhaps the most important element in a young scientist's education is the systematic assimilation of the fundamental facts and ideas of nature, and sufficient practice in their use to enable him to express them in a quantitative manner. It seems self-evident that any presentation of the foundations of science should give emphasis to the quantitative study and practice of mechanics, including statics, dynamics and hydrodynamics, so that mechanical knowledge and insight become an integral part of the man. It may be even more important to stress the fundamental facts of electricity, including potential theory, electrostatics, electrodynamics and electronics, making sure that enough practical problems are given to enable a student to consolidate his understanding of the basic methods.

Because so many of the experimental procedures in applied physics reduce ultimately to some sort of a problem in electrical measurements, it seems desirable that thorough courses in electrical measurements be given, including an introduction to the electrical characteristics of electronic, magnetic or other devices whose voltage-current relationships are nonlinear. Electrostatic measurements should not be ignored. The properties of light and wave propagation, in both homogeneous and inhomogeneous mediums, are important, and a student should be able to apply his knowledge of light waves to analogous problems in other fields. The demand today is for physicists and engineers who have *superlative training in the fundamentals of classical physics and sufficient experience and judgment to apply this training to practical problems*. Any course of study that broadens the understanding of basic physical phenomena or that provides practice in expressing them in quantitative terms is always of value.

Another important element in the training of a competent research man is the achievement of both facility with mathematical methods and skill in the physical interpretation of mathematical results. Many men have remarkable ability for formal mathematical procedures and, if they start on a sound physical basis, can deduce important results. Others fall in love with the elegance of mathematical statement and physically lose their way. It is altogether too common for experienced mathematicians to pub-

lish papers on physical phenomena that have no foundation in fact, and yet have such detail and mathematical precision that even physical scientists are temporarily mislead and scientific understanding of the problem is delayed for many years. Since so many important physical problems lie on the borderline of other sciences and since the quantitative interpretation of these problems is necessary for the guidance of scientific progress, I feel that the coordination of mathematical ability with physical understanding is most essential. Now, because preliminary estimates in physical research are the guideposts to successful experimentation, a man must further have the ability to make rapid preliminary quantitative estimates and be skilled in selecting the correct simplifications and approximations. An investigator unable to predict the approximate magnitude of an expected effect usually wastes time even though he may be a hard worker.

Every large research laboratory has at least one experimental genius, perhaps without even a high school education, who has such a well-developed intuition or feeling for physical phenomena that he is quite competent to steer a research program. This valuable insight can be acquired by practice and indeed it is often highly developed as a means for compensating a poor mathematical training. The student of physics, therefore, should be encouraged to study diverse types of physical phenomena with such care and understanding that he acquires the ability to "see" or imagine in rough quantitative terms the detailed interactions or geometric changes which occur in a physical system. The American physicist now excels in this skill while it is notably lacking in many Europeans. Principally for this reason I doubt the wisdom of the present university emphasis on Europeans and European instructional methods. If the current trend is continued many more years, the American physicist will largely lose this valuable birthright.

Outstanding experimental ability is the one quality for which an employer is ever on the alert. College and university training already aids materially in the development of experimental skill by providing the student with laboratory facilities and competent guidance. The opportunity to work in an enthusiastic group carrying forward complicated measurements of, say, atomic properties really helps to develop experimental ability and simultaneously advances the frontiers of science. The kind of experimental problem worked on is usually of no great importance, but I think that most employers regret that so many men get training in so few specialized fields. Fifteen years ago it was fashionable for university experimental training to be in spectroscopy; today it is in nuclear physics. Both are important and both are useful in training men; but the experience obtained is distressingly uniform. An entirely parallel situation has grown up in theoretical physics. Such fads are the curse of American physics, both as a science and as a profession, not because they are intrinsically bad but because they warp and limit the perspective, and crowd out the diversity of interest in science which many physicists consider important. I should like to see the science departments reduce, but not eliminate, the attention devoted to fashionable physics and broaden their interest by encouraging more experimental research in borderline and classical problems. Such research may be expensive, and require much skill and ingenuity, but it will develop experimental ability and originality of many types. The research laboratory is usually able to develop a man's experimental ability and judgment, but progress invariably is slow and considerable previous experience in a university adds notably to a man's value.

A useful investigator has a reading knowledge of the important foreign languages, knows how to search the literature, including that of patents, and is able to coordinate his experimental program with the information so obtained in such a way as to avoid the errors of earlier work. He carries out the program, digests the information obtained, and must be able to organize the results into a concise and understandable report. These reports serve, among other things, to inform the supporting organization of the work accomplished by the laboratory.

It has been noticed that some employees are sufficiently enthusiastic and interested in physics to carry on systematic study after they leave the university. Such men grow steadily in professional ability and general usefulness. After a few years they outstrip in performance their less

enthusiastic colleagues. If there were some way that the colleges could promote this habit of self-education, I feel sure that the employer would be glad to provide further stimulation. This is one phase of the educational system that deserves further university consideration.

Many qualities determine the usefulness of a scientist, but I would stress the fact that even though a man may have an altogether admirable education, much native ability, a good personality and abounding health, yet these characteristics alone, if they are not given wise and systematic direction, cannot fulfill all requirements. Too much research work is supported in this country that is ineffective. Too many investigations are made and reports written that add little or nothing to the store of human knowledge because the objectives were not well-defined in terms of already known facts. Organizations engaged in studies bordering the frontiers of knowledge are particularly vulnerable to large-scale waste and ineffectiveness. There is often too much random industry and too little systematic progress toward a worthwhile objective. In order to be of greatest value to a research laboratory, a man must have the perspective, the experience and the judgment that will enable him to make sound decisions and to direct his energy and his education toward worthwhile goals which represent true advancement in understanding. It is admitted that a laboratory need not have very many scientists of the above-mentioned caliber, but a wise administration usually looks to the future and selects men who appear to be capable of developing into exceptionally valuable employees.

In considering those qualities which determine the competence of a research physicist, I have always been impressed by the advantages of an early start. As a general thing, we have observed that a graduate of a first-class electrical engineering school has had the best opportunity to develop himself along the lines which I have pointed out as important. In the first place, he is required to complete nearly as many hours of work for a Bachelor's degree as an Arts student completes for a Master's degree. Invariably he has had a fair mathematical training and sufficient practice to consolidate it. In his basic engineering courses he has had an opportunity to develop somewhat his powers of interpretation. The engineering student has had a good deal of experimental training prior to securing even his Bachelor's degree. He has usually had laboratory and shop work or other allied experience. As a general thing he has been introduced to design work and has had to face the consequences of his errors. All in all, the training, judgment and perspective of such a man when he completes his four-year course is quite superior, provided he has made full use of his opportunities. It seems clear that such training is extremely valuable as a prelude to advanced work in physics. With such a background his experience in physics becomes a further test of his engineering ability and he approaches the experimental work from a point of view much different from the student having no engineering training. Advanced work in physics, built on such a foundation, is of the utmost value in enhancing a man's usefulness in applied physical research. The advanced work increases his facility with mathematical procedures, gives him experience in the interpretation of physical processes and tests his judgment. It must be recognized, too, that a young engineer is more likely to become deeply interested in applied physical research than a man trained in the more philosophical branches of science. Other factors being equal, I always prefer students who have had electrical engineering at a first-class college and sufficient interest in physical problems to have completed an advanced degree in physics.

On the basis of actual contacts with a large number of men who have traversed this type of training, I should like to make a strong plea to the teachers of electrical and mechanical engineers that they encourage their better students to follow through with advanced work in physics. I feel certain that the research laboratories of this country now and after the war will be able to absorb in constructive positions a large share of the men with such training. Of course, these selected men must also have the desirable personal characteristics which we have emphasized earlier, in order that their talents may be most effectively used.

Although I have dwelt on the advantages of an engineering training it must be recognized that the ultimate objectives—scientific ability,

judgment and knowledge—can be achieved by other paths. Indeed, the employer prefers to select a man for a specific job from several candidates with widely different backgrounds. Finally, I suspect that the quality of the instruction and the ability of the teacher to inspire and stimulate interest in the problems of science, plays a much more important role in the training of a physicist than any particular course he may have pursued.

Training of Physicists for Work in the Field of Applied Geophysics

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I SHALL discuss a four-year plan of college instruction which should properly train and educate a student who wishes to enter the field of applied geophysics. It is a relatively simple plan since its purpose contemplates only two major requirements: first, that the student receive thorough instruction and training in the fundamental subjects which are essential to his endeavor; second, that such instruction and training be administered in a manner which will permit the student to realize the economic significance of his endeavor.

In the matter of the first requirement, there is no essential difference between the instruction and training the student will receive, and that which would be required if he were planning to enter the academic or research field. Thorough groundwork is more important than all the special work a student can hope to do in a usual four-year college course.

Many mathematicians, physicists and chemists have found it difficult to secure a place for their services in the industrial field; and this can generally be ascribed to the lack of understanding on the part of such scientists of the value of time in this modern age, and likewise to their failure to discriminate between worthwhile endeavor and endeavor of little consequence; therefore, the second requirement of my plan, the purposes of which are to teach the student the value of time and to develop in him a high degree of personal efficiency. If success is obtained in this phase of the training, the student not only will acquire greater knowledge during the time he spends in college but, once out of college, will find it much easier to apply such knowledge effectively. The plan here considered is one that is specifically recommended for the training of physicists who desire to enter the field of applied geophysics, but for all practical purposes such a plan may be implemented as a means for acquiring knowledge and training in any of the sciences.

In this connection it might be well to digress for a moment to consider definitions. Since it is the acquisition of knowledge—certain knowledge —with which we are here concerned, let us consider the meaning of this term. *Knowledge* has been defined by Webster as, "Familiarity gained by actual experience; practical skill; technical acquaintance. . . . Instruction; enlightenment; learning; also broadly, the sum of information conserved by civilization." Consider next the difference between knowledge and wisdom. Whereas knowledge denotes acquaintance with, or clear perception of, facts, *wisdom* has been defined as, "the capacity of judging soundly and dealing broadly with facts, especially in relation to life and conduct;" and it often implies depth of insight and ripeness of experience.[1]

Now, we have to consider the term, *science.* As we know *science* in modern usage, it is "exact, organized and classified *knowledge;*" and we consider it the business of our universities

[1] Thus, "*Knowledge* and *intelligence* . . . alone do not constitute *wisdom;* the word implies the application of the fittest means to the best end." (*T. Gray.*) Again, "*Wisdom* consisteth not in knowing many things, nor even in knowing them thoroughly, but in choosing and in following what conduces most certainly to our lasting happiness and true glory." (*Landor.*) Therefore, "*Knowledge* dwells in heads replete with the thoughts of other men, *wisdom* in minds attentive to their own." (*Cowper.*)

and colleges to impart solid knowledge. According to Webster, science is "any branch or department of systemized knowledge considered as a distinct field of investigation or object of study" or, more narrowly, it is "A branch of study which is concerned with observation and classification of facts, especially with the establishment (and, strictly, the quantitative formulation) of verifiable general laws, chiefly by induction and hypotheses."

But, what is the difference between science and art? *Art* has been defined as, "skill, dexterity, or the power of performing certain actions, acquired by experience, study, or observation." It is the *knack* of a thing.[2]

As for *mathematics*, it is, according to Webster, "That science . . . which treats of the exact relations existing between quantities or magnitudes and operations, and of the methods by which, in accordance with these relations, quantities sought are deducible from others known or supposed; . . . ." In other words, mathematics is the science of order! It is our only exact science, and therefore basic to all other sciences.

In reviewing the history of the sciences, we find that the business of gathering data and information has constituted no mean portion of the scientific endeavor which has been prosecuted by man. We also observe that the opportunity for reasoning increased in direct ratio with the increase of the amount of data and information gathered and made available; and we find that all of the sciences are related in that all are dependent upon the one fundamental and primary science, mathematics.

Since mathematics is fundamental and basic, and therefore may be designated as primary, the further classification of the other sciences as *secondary*, *tertiary*, *quarternary*, and so on, is at once evident. Physics and chemistry fall in the category of *secondary* sciences, while geology, geophysics and the biological sciences take their place among the *tertiary*. To illustrate this point, it is a well-known fact that the physicists and chemists cannot very well conduct their work without a prior knowledge of mathematics; and likewise geologists and biologists are obliged to utilize a prior knowledge of mathematics, physics and chemistry.

It appears to me that the logical procedure for instructing a student who desires to become proficient in the sciences of geology and geophysics requires that his instruction in mathematics should precede all other types of instruction. If we assume that a student to be adequately equipped for work in geophysics should have mathematics through differential equations, advanced college physics—including sound, heat, light and electromagnetic theory—chemistry through advanced inorganic and, at least, elementary organic chemistry, it is easily seen that, if the student should take these courses to the fullest extent required in each subject, and in sequence, he would not be able to complete his work in four years. The program which I have to suggest will permit him to complete his work in the usual four-year college course, and it does not differ too greatly from that recommended by most colleges for training scientific students.

Upon entering college a student should commence his mathematics courses promptly; and, after having had sufficient mathematical preparation for elementary physics and chemistry, should then begin such work, taking it concurrently with his second courses in mathematics. Once having completed the elementary courses in chemistry and physics, he can commence elementary geology and geophysics and study them concurrently with the more advanced courses in chemistry and physics.

To illustrate this procedure, a student in his first year of college would study trigonometry, analytic geometry and differential calculus concurrently with a first course in general chemistry. During his second year he would take integral calculus, differential equations and the first course in physics. During his first two

[2] "A *science* teaches us to *know*, and an *art* to *do*, and all the more perfect sciences lead to the creation of corresponding useful *arts*. *Astronomy* is the foundation of the *art* of navigation; . . . *chemistry* is the basis of many useful *arts*." (*Jevons.*) Again, "*Rhetoric*, here called an *art*, is sometimes defined as a *science*. Both designations are true. . . . *Science* is systemized *knowledge;* if then the laws and principles of discourse are exhibited in an ordered and interrelated system, they appear in the character of a *science*. *Art* is *knowledge* made efficient by skill; if then rhetorical laws and principles are applied in the actual construction of discourse, they become the working rules of an *art*." (*J. F. Genung.*)

years he should take at least two years of English and two of foreign languages—perhaps one year each of French and of German—taught with the view of giving him a reading knowledge rather than a speaking knowledge of these languages. Although this would almost fill his program for his first two years, he might also be given such laboratory courses as manual training and mechanical drawing.

Throughout the third and fourth years, he should continue work in physics and, in the first term of the third year, take his first course in geology. With one year of chemistry, one year of physics and two years of mathematics behind him, he can learn more geology in six weeks than a freshman student could hope to learn in a whole year! The same idea applies to the attempts to teach physics to science majors who have no knowledge of differential calculus. I think one of the gravest mistakes made in university instruction is in attempting to teach a freshman student scientific subjects which have basic to them other sciences about which he knows little or nothing. If the student is pushed along with his mathematics as rapidly as possible, and is allowed to defer his instruction in physics until after he has received an adequate mathematical foundation, at the end of four years he will have accomplished much more scientifically and also will have had much more time for his classical work. By keeping the training in mathematics well ahead of that in the secondary sciences, the student's appetite for the latter is whetted and he undertakes their study with an assurance and avidity that is not frequently found among freshmen.

In the last half of the junior year the student may replace mathematics with an advanced course in inorganic chemistry, continue his work in geology and physics, and take a subject in the social sciences. In the senior year he can continue work in physics and advanced geology, and take a year of organic chemistry and an elective course either along the lines of his major subject or in the social sciences. Such a student will then be a trained scientist with an unusual mastery of mathematics, chemistry and physics for an undergraduate; and, at the same time, he will not have neglected his general education.

After the student has graduated from college and is ready to enter the field of applied science, he still has a great deal to learn which concerns the relation between the cost of the work he is about to undertake and the probable value of such work. That is, he still has to learn the exercise of *wisdom*.

A student who has been instructed and trained in either the afore-described manner or in the manner usually prescribed by most colleges will be found extremely impractical. He will have little sense of the value of his time, and less concerning the value of the time of his associates. He will probably have no idea of the worth of an undertaking and little or no conception as to whether his efforts will prove profitable to the industry into which he has entered.

Therefore, it is highly desirable that an effort be made in college to give the student a practical point of view. Various methods can be devised and utilized to attain this result. For instance, the system employed in conducting laboratory work can readily be altered for this purpose. In assigning various laboratory experiments, time limits for performing the work should be set, and these limits should vary over a wide range in order that the student may be impressed with the idea that one experiment is of much greater consequence than another; certainly when he undertakes practical work in the industrial field, he will find numerous endeavors that are worth no more than a few hours of his time, whereas there will be others that may be worth several years. By acquiring fine conceptions of the value of time and the potential value of undertakings, the student may develop into an efficient worker who not only can utilize his own time to great advantage but can direct the use of the time of others to equal advantage.

Why do we find that a man who has attended an engineering school and has received instruction and training in physics and chemistry often proves more adaptable to industrial work than the man who has been trained as a scientist in a college of arts and sciences? The answer to the question is simply this: General training in the engineering school is of such a nature as to develop in the student the practical point of view and, hence, concepts of the cost of time

and materials and of the probable value of the project in hand. The training of a student in any science should be conducted so that he will acquire a good understanding of values in connection with his work.

I have proposed a plan for training physicists for work in applied geophysics. For training any other scientist I would recommend exactly the same plan, except that the science courses should point into the direction of the particular field of endeavor. By far the bulk of the college work of the future scientist will consist of sound basic training in the three fundamental sciences —mathematics, physics and chemistry; yet a substantial amount of his time will be devoted to securing a general education by studying history, languages and the social sciences.

If you will show me a college graduate who, in his four years' work, has been thoroughly grounded and trained in the manner I have prescribed and recommended, who has had instilled in him a practical point of view consisting of an appreciation of the value of time and the cost of materials, and who has acquired the ability to estimate the probable value of his undertakings, I will show you a man who has both knowledge and wisdom and who will succeed admirably in the field of petroleum geophysics, whether he be employed in the laboratory or in the field.

The War Policy Committee of the American Institute of Physics

THE American Institute of Physics has established a War Policy Committee for Physics, composed of O. E. BUCKLEY, K. T. COMPTON, H. L. DODGE, R. C. GIBBS and P. E. KLOPSTEG, *Chairman*. The Committee issued its first report on May 1, 1942.

Because of general confusion concerning the meaning of the term "physicist," the Committee adopted and recommended to the Founder Societies of the Institute for adoption the following definitions: (a) a ***physicist*** is one whose training and experience lie in the study and applications of the interactions between matter and energy in the fields of mechanics, acoustics, optics, heat, electricity, magnetism, radiation, atomic structure and nuclear phenomena; (b) to qualify as a ***professional physicist*** one must have had at least eight years of training and experience in physics. Toward this experience four years of formal collegiate education with major emphasis on physics may be credited, year for year, if it leads to a bachelor's degree, five years if it leads to a master's degree, and seven years if it leads to a doctor's degree, from a recognized institution. Years of teaching of physics in a recognized institution may be credited as years of experience. By a ***recognized institution*** is meant one that appears in the list of institutions approved by the Association of American Universities.

Persons trained in physics at all levels, from secondary school graduates to doctors of philosophy, are urgently needed. The need for those having five or more years of experience in physics, as defined in (b) above, is now growing at the rate of 1500 to 2000 per year and the current annual supply appears to be no more than 500. It is officially estimated that more than 100,000 individuals are needed who have sufficient preparation in mathematics and physics for training in technical war operations.

The Committee therefore urges all secondary schools, colleges and universities to recommend immediately that students with natural aptitude for these studies enrol in courses in fundamental physics and their prerequisites in mathematics, whatever the stage in the student's educational career. Secondary schools should emphasize mathematics through trigonometry; training in physics should be in the fundamentals of the subject. Colleges and universities are urged to provide the most advanced courses for which students are qualified and, where facilities are suitable, to establish intensive courses, with laboratory practice, in electronic devices and high frequency circuits.

The Committee would strongly endorse the adoption of any policies and regulations by the Selective Service System, personnel divisions of the Armed Services and other government agencies concerned, which would ensure that every available individual trained in physics be placed in a position in which full use is made of that training.

A Gyroscope Experiment for the Dynamics Laboratory

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ONE of the most interesting and instructive motions of a rigid body is that of a symmetrical rotator supported at one point, such as a gyroscope in the usual Cardan support. The usual practice is to discuss the theory of the gyroscope and then perform qualitative demonstrations of various types, but it should prove very instructive to have the students perform some quantitative experiments in the laboratory as well. It was with this end in view that the present experiment was developed.

A torque applied to a symmetric rotator in a Cardan support produces a state of steady precession. The torque can be produced by means of a weight applied at a finite distance from the center of mass. On application of Newton's laws of motion the result is

$$mgs = \omega_p(I\omega_s - J\omega_p \cos\theta). \qquad (1)$$

Here mg is the weight; s is the distance from the center of mass of the rotator to the point of intersection of the line of action of the weight and the axis of spin; ω_p and ω_s are the angular velocities of precession and spin, respectively; I and J are the moments of inertia about the axis of spin and an axis through the fixed point perpendicular to the spin axis; and θ is the angle that the axis of spin makes with the vertical.[1]

In a case of considerable practical importance the two moments of inertia I and J are of the same order of magnitude while the angular velocity of spin ω_s is very large compared to the angular velocity of precession ω_p. For this important case Eq. (1) becomes, to a very good approximation,

$$mgs = I\omega_p\omega_s. \qquad (2)$$

The approximation becomes even better if the axis of spin is close to the horizontal position, since $\cos\theta$ is then nearly zero.

The purpose of the experiment is to measure mgs and compare the result with a measured value of $I\omega_p\omega_s$. Figure 1, which is a schematic diagram of the experimental arrangement, is largely self-explanatory, but a few points may need elaboration. A certain amount of care must be used in the construction of the intermittent

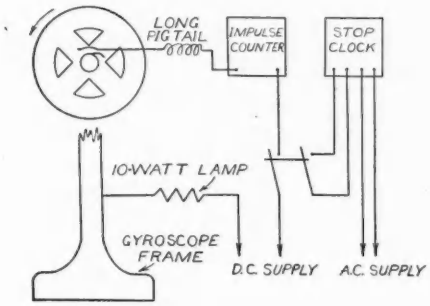

FIG. 1. Schematic diagram of the apparatus.

rotor contact. The important point is to maintain the contact for a considerable fraction of a revolution, say about one-third; otherwise the impulse counter may not respond each time, with the result that it will record too few revolutions. The contact that is stationary with respect to the gimbal ring supporting the rotator must of course be insulated from the ring. Ordinary friction tape serves very well. This contact may be made from a small strip of brass bent into a circular arc near the end, as indicated in Fig. 1. The lead from the insulated contact to the impulse counter should be rather fine wire, about No. 30, wound into a long pigtail and fastened to a support located about a meter or so directly above the gyroscope. This method of bringing out the lead insures positive contact at all times, exerts no appreciable force on the rotator, is easy to construct and is less bother than a slip ring or mercury trough. The pigtail connection will of course twist off after a time, but it is extremely easy to replace. The rotating contact may be a short piece of lamp cord soldered to the shaft and bent into the proper shape so as to

[1] See, for example, Synge & Griffith, *Principles of mechanics* (McGraw-Hill, ed. 1), p. 417.

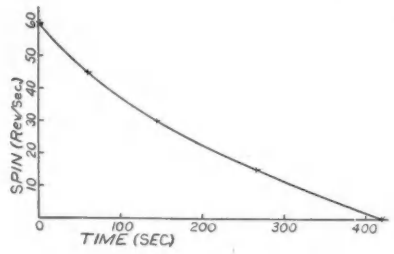

FIG. 2. Chronograph curve of spin *versus* time.

brush along the circular arc of the insulated contact.

The method of operation is extremely simple. One merely closes the single-throw, double-pole switch, thereby simultaneously starting the impulse counter and the electric stop clock. After the system has made one or more revolutions in precession the switch is opened. The impulse counter has registered the number of revolutions of the spinning rotator during the time interval registered on the stop clock. From these data average angular velocities of spin and precession may be computed. Equation (2), however, calls for instantaneous values of these velocities. These are obtained from the average values as follows. The average values are the same as the corresponding instantaneous values at the midpoint of the time interval provided that both angular velocities are linear functions of the time. This is very nearly true if the time intervals are not too long, which will be the case if one restricts himself to timing one, or possibly

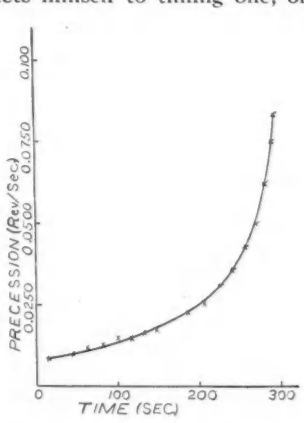

FIG. 3. Chronograph curve of precession *versus* time.

two, revolutions of precession. The curves of Figs. 2 and 3 clearly show that for small time intervals such as those used in the experiment, both velocities may be considered linear functions of the time to a very good approximation. This holds for both cone and ball bearings. Figure 2 was obtained by means of a chronograph and neon stroboscope flasher. Figure 3 was obtained by keying the chronograph at the end of each quarter turn of precession and plotting average values of the corresponding angular velocity during each of these quarter turns as a function of the time. Both curves were obtained by students.

The moment of inertia I of the gyroscope wheel may be easily obtained by means of an auxiliary experiment. Perhaps the easiest method, if the rotator has spokes, is to dismount the rotator, hang it from a knife-edge support and find its period of oscillation as a pendulum. The moment of inertia of the rotator may then be computed from its measured period, mass and distance from the knife-edge to the center. Another common method is to use a moment of inertia torsion table.

A typical set of data with calculated results obtained by students in the intermediate course in dynamics at Rensselaer Polytechnic Institute is given in Table I. The average value of $\omega_p\omega_s$ for

TABLE I. Set of data with calculated results.

Mass M of rotator, 1291 gm; inside diameter $2r$ of rim of rotator, 9.84 cm; period of oscillation T of the rotator as a pendulum, 0.6805 sec; therefore, $I=(MgrT^2/4\pi^2)-Mr^2=41{,}800$ gm cm^2.

Mass m, 227.1 gm; distance s, 5.98 cm; therefore, $mgs=1.33\times10^6$ dyne cm.

Number of Counts, N	Stop Clock, t (sec)	$\omega_p[=2\pi/t]$ (sec^{-1})	$\omega_s[=2\pi N/t]$ (sec^{-1})	$\omega_p\omega_s$ (sec^{-2})
70	9.33	0.675	47.2	31.8
75	9.67	0.651	48.8	31.8
60	8.63	0.728	43.6	31.8
156	14.10	0.443	69.5	30.8
143	13.55	0.464	66.3	30.8
192	15.65	0.410	77.0	31.6
125	12.70	0.494	61.9	30.6
137	13.21	0.476	65.6	31.2
241	17.74	0.354	85.2	30.2

the nine runs is 31.3 sec^{-2}, and the average value of $I\omega_p\omega_s$ for these runs is 1.32×10^6 dyne cm. This is equal to the value of mgs—1.33×10^6 dyne cm—to well within the experimental error.

Development of Differential Operators by a Method of Geometric Projection

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THE method of development of differential operators as outlined here, while not essentially new, is so simple that attention should be called to it.

Let ξ, η, ζ be orthogonal curvilinear coordinates and let λ, μ, ν be the quantities by which they must be multiplied to change them to distances. The operators $\partial/\lambda\partial\xi$, $\partial/\mu\partial\eta$, $\partial/\nu\partial\zeta$ can be represented geometrically by arrows from the point ξ, η, ζ in the directions of increasing values of ξ, η, ζ, respectively. If $\alpha_1, \beta_1, \gamma_1$; $\alpha_2, \beta_2, \gamma_2$; $\alpha_3, \beta_3, \gamma_3$ are direction cosines of the respective operators, then, by taking projections upon the x, y, z axes, we obtain

$$\begin{aligned}\partial/\partial x&=\alpha_1\partial/\lambda\partial\xi+\alpha_2\partial/\mu\partial\eta+\alpha_3\partial/\nu\partial\zeta,\\ \partial/\partial y&=\beta_1\partial/\lambda\partial\xi+\beta_2\partial/\mu\partial\eta+\beta_3\partial/\nu\partial\zeta,\\ \partial/\partial z&=\gamma_1\partial/\lambda\partial\xi+\gamma_2\partial/\mu\partial\eta+\gamma_3\partial/\nu\partial\zeta.\end{aligned}\tag{1}$$

These expressions can be written down by inspection from Fig. 1.

Likewise by inspection we can write

$$\begin{aligned}\partial/\lambda\partial\xi&=\alpha_1\partial/\partial x+\beta_1\partial/\partial y+\gamma_1\partial/\partial z,\\ \partial/\mu\partial\eta&=\alpha_2\partial/\partial x+\beta_2\partial/\partial y+\gamma_2\partial/\partial z,\\ \partial/\nu\partial\zeta&=\alpha_3\partial/\partial x+\beta_3\partial/\partial y+\gamma_3\partial/\partial z.\end{aligned}\tag{2}$$

Remembering that ξ, η, ζ as well as x, y, z are orthogonal coordinates, we can obtain Eqs. (2)

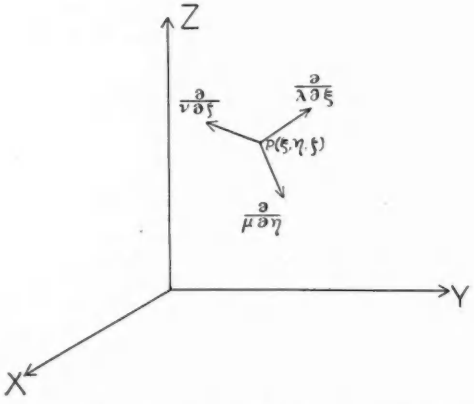

FIG. 1. The vector operators in general coordinates projected upon the Cartesian axes.

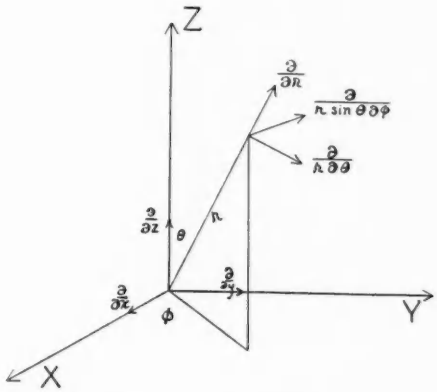

FIG. 2. The operators in spherical polar coordinates.

also by solving the simultaneous Eqs. (1) for the differential operators with respect to ξ, η, ζ.

From the foregoing considerations, operators of higher orders can be derived; thus

$$\begin{aligned}\frac{\partial^2}{\partial x^2}&=\left(\alpha_1\frac{\partial}{\lambda\partial\xi}+\alpha_2\frac{\partial}{\mu\partial\eta}+\alpha_3\frac{\partial}{\nu\partial\zeta}\right)\\ &\quad\times\left(\alpha_1\frac{\partial}{\lambda\partial\xi}+\alpha_2\frac{\partial}{\mu\partial\eta}+\alpha_3\frac{\partial}{\nu\partial\zeta}\right)\\ &=\alpha_1\frac{\partial}{\lambda\partial\xi}\left(\alpha_1\frac{\partial}{\lambda\partial\xi}\right)+\alpha_1\frac{\partial}{\lambda\partial\xi}\left(\alpha_2\frac{\partial}{\mu\partial\eta}\right)\\ &\quad+\alpha_1\frac{\partial}{\lambda\partial\xi}\left(\alpha_3\frac{\partial}{\nu\partial\zeta}\right)+\alpha_2\frac{\partial}{\mu\partial\eta}\left(\alpha_1\frac{\partial}{\lambda\partial\xi}\right)\\ &\quad+\alpha_2\frac{\partial}{\mu\partial\eta}\left(\alpha_2\frac{\partial}{\mu\partial\eta}\right)+\alpha_2\frac{\partial}{\mu\partial\eta}\left(\alpha_3\frac{\partial}{\nu\partial\zeta}\right)\\ &\quad+\alpha_3\frac{\partial}{\nu\partial\zeta}\left(\alpha_1\frac{\partial}{\lambda\partial\xi}\right)+\alpha_3\frac{\partial}{\nu\partial\zeta}\left(\alpha_2\frac{\partial}{\mu\partial\eta}\right)\\ &\quad+\alpha_3\frac{\partial}{\nu\partial\zeta}\left(\alpha_3\frac{\partial}{\nu\partial\zeta}\right).\end{aligned}\tag{3}$$

Similarly the expressions for the other second-order operators follow. These derivations become

somewhat tedious, although the procedure is straightforward and elementary.

In order to illustrate the ease with which the linear operators can be developed, we shall now apply the method to a few specific cases. Let us consider the relation between rectangular Cartesian coordinates and spherical polar coordinates. The orientation is given in Fig. 2. It is to be remembered that, in taking projections, the final result is the same no matter what intermediate steps are taken. It is simpler for our procedure to project first upon an intermediate line and from there to the final coordinate.

By inspection from Fig. 2 we write down immediately

$$\begin{aligned}\partial/\partial x &= \sin\theta\cos\varphi\,\partial/\partial r + \cos\theta\cos\varphi\,\partial/r\partial\theta \\ &\quad - \sin\varphi\,\partial/r\sin\theta\partial\varphi, \\ \partial/\partial y &= \sin\theta\sin\varphi\,\partial/\partial r + \cos\theta\sin\varphi\,\partial/r\partial\theta \\ &\quad + \cos\varphi\,\partial/r\sin\theta\partial\varphi, \\ \partial/\partial z &= \cos\theta\,\partial/\partial r - \sin\theta\,\partial/r\partial\theta.\end{aligned} \tag{4}$$

We can also see by inspection that

$$\begin{aligned}\partial/\partial r &= \cos\varphi\sin\theta\,\partial/\partial x + \sin\varphi\sin\theta\,\partial/\partial y \\ &\quad + \cos\theta\,\partial/\partial z, \\ \partial/r\partial\theta &= \cos\varphi\cos\theta\,\partial/\partial x + \sin\varphi\cos\theta\,\partial/\partial y \\ &\quad - \sin\theta\,\partial/\partial z, \\ \partial/r\sin\theta\partial\varphi &= -\sin\varphi\,\partial/\partial x + \cos\varphi\,\partial/\partial y.\end{aligned} \tag{5}$$

It is seen that ξ, η, ζ become r, θ, φ; that λ, μ, ν become 1, r, $r\sin\theta$; and that $\alpha_1, \beta_1, \gamma_1; \alpha_2, \beta_2, \gamma_2; \alpha_3, \beta_3, \gamma_3$ become $\cos\varphi\sin\theta$, $\sin\varphi\sin\theta$, $\cos\theta$; $\cos\varphi\cos\theta$, $\sin\varphi\cos\theta$, $-\sin\theta$; $-\sin\varphi$, $\cos\varphi$, 0, respectively, in relation to the general case.

If second-order operators are desired, they can be developed as has been indicated. The procedure is elementary. If we wish to develop the Laplacian $\partial^2/\partial x^2+\partial^2/\partial y^2+\partial^2/\partial z^2$, it is best to introduce the intermediate coordinate ρ as shown in Fig. 3.

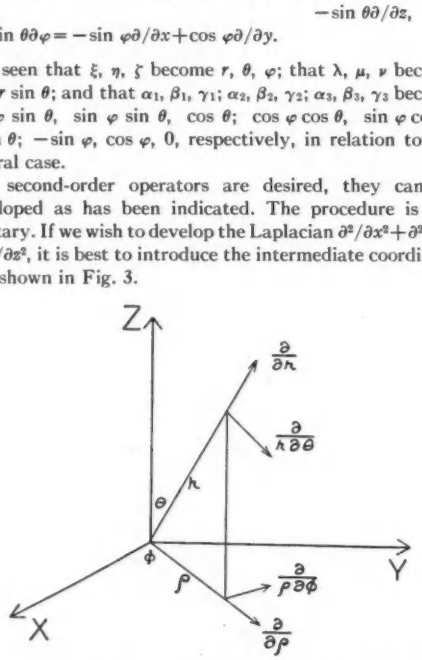

FIG. 3. The relation between cylindrical and spherical polar coordinates.

In the x, y plane,

$$\begin{aligned}\partial/\partial x &= \cos\varphi\,\partial/\partial\rho - \sin\varphi\,\partial/\rho\partial\varphi, \\ \partial/\partial y &= \sin\varphi\,\partial/\partial\rho + \cos\varphi\,\partial/\rho\partial\varphi,\end{aligned}$$

$$\partial^2/\partial x^2 = \left(\cos\varphi\frac{\partial}{\partial\rho} - \sin\varphi\frac{\partial}{\rho\partial\varphi}\right)\left(\cos\varphi\frac{\partial}{\partial\rho} - \sin\varphi\frac{\partial}{\rho\partial\varphi}\right)$$

$$= \cos^2\varphi\frac{\partial^2}{\partial\rho^2} + \frac{2\cos\varphi\sin\varphi}{\rho^2}\frac{\partial}{\partial\varphi} - \frac{2\cos\varphi\sin\varphi}{\rho}\frac{\partial^2}{\partial\rho\partial\varphi} + \frac{\sin^2\varphi}{\rho}\frac{\partial}{\partial\rho} + \frac{\sin^2\varphi}{\rho^2}\frac{\partial^2}{\partial\varphi^2}, \tag{6}$$

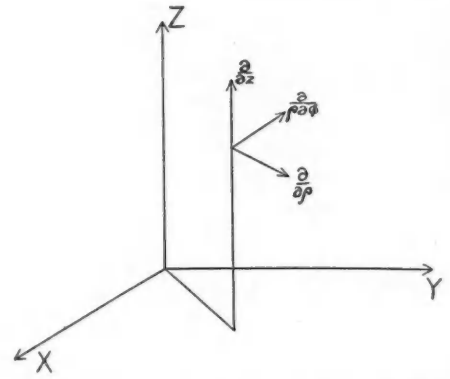

FIG. 4. The vector operators in cylindrical coordinates.

$$\partial^2/\partial y^2 = \left(\sin\varphi\frac{\partial}{\partial\rho} + \cos\varphi\frac{\partial}{\rho\partial\varphi}\right)\left(\sin\varphi\frac{\partial}{\partial\rho} + \cos\varphi\frac{\partial}{\rho\partial\varphi}\right)$$

$$= \sin^2\varphi\frac{\partial^2}{\partial\rho^2} - \frac{2\sin\varphi\cos\varphi}{\rho^2}\frac{\partial}{\partial\varphi} + \frac{2\sin\varphi\cos\varphi}{\rho}\frac{\partial^2}{\partial\rho\partial\varphi} + \frac{\cos^2\varphi}{\rho}\frac{\partial}{\partial\rho} + \frac{\cos^2\varphi}{\rho^2}\frac{\partial^2}{\partial\varphi^2}.$$

Combining these two equations, we obtain

$$\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} = \frac{\partial^2}{\partial\rho^2} + \frac{1}{\rho}\frac{\partial}{\partial\rho} + \frac{1}{\rho^2}\frac{\partial^2}{\partial\varphi^2}. \tag{7}$$

By analogy, we have immediately from the ρ, z plane,

$$\frac{\partial^2}{\partial z^2} + \frac{\partial^2}{\partial\rho^2} = \frac{\partial^2}{\partial r^2} + \frac{1}{r}\frac{\partial}{\partial r} + \frac{1}{r^2}\frac{\partial^2}{\partial\theta^2}. \tag{8}$$

Addition of these two results gives

$$\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2} = \frac{1}{\rho}\frac{\partial}{\partial\rho} + \frac{1}{\rho^2}\frac{\partial^2}{\partial\varphi^2} + \frac{\partial^2}{\partial r^2} + \frac{1}{r}\frac{\partial}{\partial r} + \frac{1}{r^2}\frac{\partial^2}{\partial\theta^2}. \tag{9}$$

By inspection we write

$$\rho = r\sin\theta, \quad \partial/\partial\rho = \sin\theta\,\partial/\partial r + \cos\theta\,\partial/r\partial\theta.$$

Substitution in Eq. (9) gives

$$\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2} = \frac{\partial^2}{\partial r^2} + \frac{1}{r\sin\theta}\left(\sin\theta\frac{\partial}{\partial r} + \cos\theta\frac{\partial}{r\partial\theta}\right) + \frac{1}{r^2\sin^2\theta}\frac{\partial^2}{\partial\varphi^2} + \frac{1}{r}\frac{\partial}{\partial r} + \frac{1}{r^2}\frac{\partial^2}{\partial\theta^2},$$

which can be reduced to

$$\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2} = \frac{1}{r^2}\frac{\partial}{\partial r}\left(r^2\frac{\partial}{\partial r}\right) + \frac{1}{r^2\sin\theta}\frac{\partial}{\partial\theta}\left(\sin\theta\frac{\partial}{\partial\theta}\right) + \frac{1}{r^2\sin^2\theta}\frac{\partial^2}{\partial\varphi^2}. \tag{10}$$

The results for cylindrical coordinates follow directly from Fig. 4. Thus,

$$\frac{\partial^2}{\partial x^2}+\frac{\partial^2}{\partial y^2}=\frac{\partial^2}{\partial \rho^2}+\frac{1}{\rho}\frac{\partial}{\partial \rho}+\frac{1}{\rho^2}\frac{\partial^2}{\partial \varphi^2},$$

so that

$$\frac{\partial^2}{\partial x^2}+\frac{\partial^2}{\partial y^2}+\frac{\partial^2}{\partial z^2}=\frac{\partial^2}{\partial \rho^2}+\frac{1}{\rho}\frac{\partial}{\partial \rho}+\frac{1}{\rho^2}\frac{\partial^2}{\partial \varphi^2}+\frac{\partial^2}{\partial z^2}. \quad (11)$$

This method is very simple compared to the usual procedure of partial differentiation and substitution from the relations connecting the two systems.

The Teaching of Simple Alternating-Current Relations Without the Explicit Use of the Calculus

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IN connection with a course on electricity and magnetism recently given a class of R.C.A.F. men, we have found it possible to derive most of the basic alternating-current relations without the explicit use of calculus. The method used assumes a knowledge of: (1) mechanical simple harmonic motion defined as the motion, along the diameter of a circle, of the projection of a particle moving in the circumference with constant speed; and (2) the fundamental ideas of electromagnetic induction, particularly the Faraday-Neumann law that an induced electromotive force is equal to the rate at which magnetic lines are cut or at which the number of flux-turns is changing. As some parts of the treatment have not been seen in any textbooks, the following outline may be of interest.

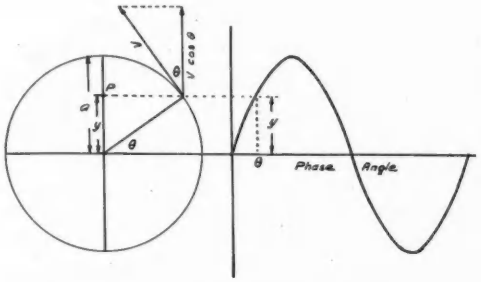

FIG. 1. Graph of a simple harmonic motion, applicable to alternating currents when current replaces displacement and velocity corresponds to time-rate of change of current.

(*a*) The application of the Faraday-Neumann law to the case of a rectangular coil rotating in a uniform magnetic field leads without difficulty to the relation $e=E_0 \sin \theta$, where e and E_0 are, respectively, the instantaneous and the maximum values of the emf.

(*b*) The average student has no difficulty in accepting the corresponding current law, $i=I_0 \sin \theta$, where i is the instantaneous value and I_0 the maximum, but even at this stage it is desirable to point out that there may be phase differences between current and voltage. The point

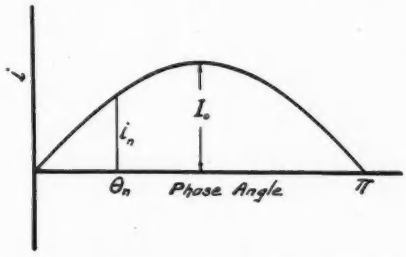

FIG. 2. Graph representing a single half-cycle of an alternating current.

may be emphasized by a simple mechanical demonstration with a pendulum or a spring supporting a mass. It is easy to show that if any periodic force is applied to such a system, the resulting motion has the same period as the force, but, in general, there is a phase difference.

(*c*) The identity between the curve representing a mechanical simple harmonic motion, $y=a \sin \theta$, and an alternating current, $i=I_0 \sin \theta$, may now be indicated and the point emphasized by the use of an oscillograph. At this stage it is convenient to emphasize that the velocity, or time-rate of change of displacement, of the vibrating particle is a maximum when the phase angle θ is 0, π, 2π, ···. For these values the velocity is the same as that of the imaginary particle in the circle of reference, and hence its magnitude is $2\pi fa$, where a is the amplitude and f the frequency. More generally, the velocity of the vibrating particle in any position, such as P, Fig. 1, is $2\pi fa \cos \theta$.

Since current changes according to the same law as displacement in a mechanical simple harmonic motion, it follows at once that the maximum time-rate of change of current is $2\pi fI_0$, and the time-rate of change of current for any phase angle θ is $2\pi fI_0 \cos \theta$.

(*d*) *Proof that the instrument current is* $I_0/\sqrt{2}$.—If the time $\frac{1}{2}T$ of a half-cycle is divided into a large number n

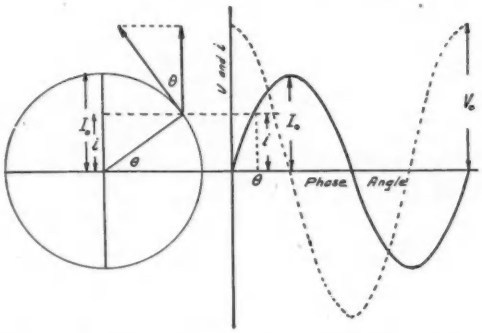

FIG. 3. Graphs showing the relation between the instantaneous value of an alternating current and the corresponding time-rate of change of current.

of equal time intervals, each of magnitude ΔT, and if the heat developed in a resistance R in the half-cycle is considered, we can write

$$I^2R\cdot\tfrac{1}{2}T=\Sigma\, i_n^2R\Delta T=\frac{RT}{2n}\Sigma\, i_n^2,$$

where I is the instrument current, or the rms value defined in the usual way, and i_n is the instantaneous current corresponding to phase angle θ. Hence

$$I^2=\frac{\Sigma\, i_n^2}{n}=\frac{\Sigma\, I_0^2\sin^2\theta_n}{n}=I_0^2\frac{\Sigma(1-\cos 2\theta_n)}{2n}$$
$$=I_0^2\frac{n-\Sigma\cos 2\theta_n}{2n}=\frac{I_0^2}{2}-I_0^2\frac{\Sigma\cos 2\theta_n}{2n}.$$

Since $\Sigma\cos 2\theta_n$ is taken over values of 2θ ranging from 0 to 360°, its value is zero, and we can write, finally, $I=I_0/\sqrt{2}$.

(*e*) *Proof that the inductive reactance is* $2\pi fL$.—Let a current $i[=I_0\sin\theta]$ exist in an inductor of inductance L henries and of negligible resistance. The applied potential difference v across the inductor at any instant is equal in magnitude to the induced emf, or

$$v=L\times(\textit{time-rate of change of current}).$$

Therefore, V_0, the *maximum* value of the applied voltage, is equal to $L\times(\textit{maximum time-rate of change of current})$; that is,

$$V_0=L\cdot 2\pi fI_0,$$

or

$$V_0/\sqrt{2}=2\pi fLI_0/\sqrt{2}.$$

Thus

$$V(\text{rms})=2\pi fLI,$$

or

$$V/I=2\pi fL.$$

Since the maximum rate of change of current occurs when $\theta=0,\ \pi,\ \cdots$, it is obvious that the phase of the corresponding voltage is 90° ahead of that of the current. More generally, we may write

$$v=L\cdot 2\pi fI_0\cos\theta=V_0\cos\theta=V_0\sin(\theta+90°),$$

a plot of which gives the dotted curve in Fig. 3.

(*f*) *Proof that the capacitative reactance is* $1/2\pi fC$.—Since current is time-rate of change of *quantity* of electricity, the same type of proof can be utilized for the derivation of the capacitative reactance and the phase difference between the current and voltage in a circuit containing capacitance.

If an alternating voltage, given by the relation $v=V_0\sin\theta$, is applied to a condenser of capacitance C, then q, the instantaneous charge on the condenser, is equal to Cv. The quantity q, or Cv, then takes the place of displacement in the s.h.m. diagram (Fig. 1).

It follows at once that

$$I_0=\textit{maximum time-rate of change of } q=2\pi fCV_0,$$

or

$$I_0/\sqrt{2}=2\pi fCV_0/\sqrt{2};$$

thus

$$I=2\pi fCV,$$

or

$$V/I=1/2\pi fC.$$

Also, the instantaneous value of the current is given by

$$i=2\pi fCV_0\cos\theta,$$

from which it is seen that the current in a circuit containing only capacitance is 90° ahead of the applied voltage.

(*g*) *More general relations.*—In deriving more general relations, such as the impedance for a circuit containing resistance and inductance, or resistance, inductance and capacitance, comparison is again made with a mechanical simple harmonic motion. By plotting two s.h.m. with the same period, but with any difference of phase, the student can see by inspection that the resultant motion is simple harmonic with the original period. By making use of the two circles of reference, he can then be readily shown the quick "vector" method of finding the resultant amplitude.

This method can then be directly applied to alternating currents. For example, the potential difference across a resistance and an inductance consists of two components that have maximum values, or amplitudes, equal to I_0R and $I_02\pi fL$, differing in phase by 90°. The vector, or clock, diagram at once gives $I_0(R^2+(2\pi fL)^2)^{\frac{1}{2}}$ as the amplitude of the resultant voltage, from which it follows that

$$V(\text{rms})=I_0(R^2+(2\pi fL)^2)^{\frac{1}{2}}/\sqrt{2}=I(R^2+(2\pi fL)^2)^{\frac{1}{2}},$$

or the impedance is

$$V/I=(R^2+(2\pi fL)^2)^{\frac{1}{2}}.$$

Determination of the Wave-Length of Light by Diffraction at Grazing Incidence

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THE method of grazing incidence is suitable for measuring a wave-length that is a small fraction of the grating space; it has been used for direct measurement on x-rays and on electrons, whose wave-lengths are small compared with the spacing of even the most closely ruled gratings. The method is well illustrated by the following laboratory experiment, in which the millimeter or half-millimeter ruling of an ordinary steel scale is used to measure the wave-length of light.

According to the elementary theory of a plane diffraction grating,

$$m\lambda = d\,(\cos\phi_0 - \cos\phi_m), \qquad (1)$$

where d is the grating constant, ϕ_0 is the complement of the angle of incidence, ϕ_m that of the angle of diffraction (Fig. 1), and m is the spectral order number. On writing down a similar equation for $n\lambda$ and subtracting it from Eq. (1), we obtain

$$(m-n)\lambda = d\,(\cos\phi_n - \cos\phi_m). \qquad (2)$$

It is instructive to write this expression in the form

$$\lambda = \frac{2d}{m-n}\sin\tfrac{1}{2}(\phi_m+\phi_n)\sin\tfrac{1}{2}(\phi_m-\phi_n). \qquad (3)$$

Equation (3) shows that, if the angle of incidence is adjusted so that $\phi_m - \phi_n$ is of the same order as $\phi_m + \phi_n$, neither of the measured angles need be very small; thus, if λ is 5×10^{-5} cm, n is 5, m is 15 and d is 0.05 cm, ϕ_n may be about 7° and ϕ_m about 10°. For elementary classes a spectrometer reading to 0.1° will give an accuracy of about 5 percent. More advanced students may obtain a higher accuracy, either by using a good spectrometer that reads to 1 min of arc or by means of a goniometer especially designed for measuring small angles.[1] Whichever method is used, the grating is a flat portion of a steel scale, selected by examining the quality of the specular reflection from the scale after it has been polished with metal polish. About 2 in. —less if the spectrometer is small—is cut off, care being taken not to distort the scale in cutting. If the millimeter ruling does not extend all the way across the scale, it is desirable to cover the remainder of the scale with dead black paint so as to reduce the scattered light; usually it will also be found advantageous to cover a little of the ruling itself near the ends. To reduce the scattered light still further, the collimator lens is covered with a mask containing a slit about 5 mm wide, and a screen is placed as shown at D in Fig. 1. Chromium-plating the scale prevents its rusting but does not improve the quality of the images because the additional light reflected goes into the zero-order spectrum.

With a strong source of light, such as a mercury arc or a sodium-vapor lamp, about 20 orders may be seen. With the mercury lamp there is considerable overlapping of orders; hence the student can learn how this overlapping may cause difficulty in the use of large gratings and how the difficulty may be avoided by means of filters. In the higher orders, the two mercury yellow lines are resolved; the value of the resolving power obtained is usually about two-thirds the theoretical limiting value mN, where m is the order and N the number of lines in the grating.

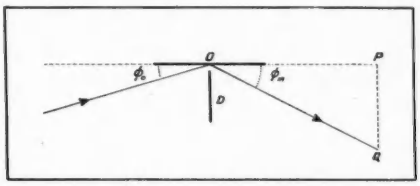

FIG. 1. Diagrammatic representation of path of a ray of light falling upon the steel scale (grating) at O. The screen D serves to exclude stray light.

If it is desired to obtain an accuracy of 1 percent without using a good spectrometer the following method may be used; it has the advantage that the student is able to build the whole apparatus for himself and see every part.

[1] Searle, *Experimental optics* (Cambridge), p. 62. This simple and elegant goniometer permits measurement to within 0.03° of angles less than 10°.

The apparatus is best set up in a darkened room. The source of light is placed behind a slit and a lens of about 4 diopters, masked with a 5-mm slit, is arranged so as to produce a narrow parallel beam of light. This beam is allowed to fall on the scale, which is soldered to a piece of metal that can be attached to a support. Another lens of 4 or 5 diopters is used to bring the reflected and diffracted beams to a focus on a white card. The specularly reflected image and the direct image—due to light which has passed above the scale–should be clearly seen. The slit is rotated until these images are parallel and a few spectra should then be visible. The second lens and the card may then be replaced by a goniometer of the type devised by Searle.[1] In this apparatus a lens and cross wire are mounted on an arm that can be rotated about an axis defined by a ball bearing resting in a hole. With the aid of a small eyepiece the cross wire can be accurately set on any line of the spectrum. The distance PQ corresponding to the setting for any line is read and, if OP is known, the ratio PQ/OP gives the value of $\tan\varphi$. The values of $\cos\varphi$ may be obtained from tables and inserted in Eq. (2), or the following formula may be used:

$$(m-n)\lambda=\tfrac{1}{2}d\frac{SD}{(OP)^2}\left(1-\frac{3}{8}\frac{S^2+D^2}{(OP)^2}\right), \qquad (4)$$

where S is the sum and D the difference of the values of PQ corresponding to the mth and nth orders for the same wave-length. Equation (4) may easily be obtained from Eq. (2) by putting $\cos\phi=OP/(OP^2+PQ^2)^{\frac{1}{2}}$ and retaining the second-order terms in a binominal expansion.

Using a simple mount for bending the steel scale into an arc of a circle, one can obtain spectra without the use of lenses and so demonstrate the focusing properties of curved gratings. The manipulation of the curved grating is more difficult and the quality of the spectra obtained is not satisfactory for measurement. However, the experiment forms an interesting demonstration for a small class. If desired, three or four orders may be photographed by bending film or sensitized paper to the appropriate curve.

A New Form of Sound Resonance Tube

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FOR some years we have used in the elementary laboratories at Rutgers University a resonance tube for the measurement of the velocity of sound in air that does not employ the usual water reservoir with its connecting rubber tube. The ordinary apparatus, besides being rather tedious to use, has several undesirable features which are sure to be present when placed in the hands of beginners. The apparatus here described has many advantages over the ordinary form. It is always set up and ready for use, is practically unbreakable and yields data quickly.

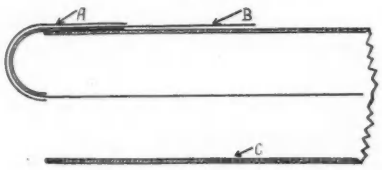

FIG. 1. *A*, small diameter tubing to serve as a guide for the control cord; *B*, control cord; *C*, resonance tube.

A brass tube about 155 cm long and 5.0 cm in diameter is mounted horizontally on a board 120 cm long and 12 cm wide. Two 10-cm lengths of brass tubing of inside diameter 2 mm are bent in the form of a U, and one of these tubes is soldered to each end of the resonance tube, as shown at A in Fig. 1. These small bent tubes serve as guides for the endless oiled-silk cord B that controls the position of the movable spool which furnishes the reflecting surface within the resonance tube. Pulleys were tried in place of the U-tubes, but they were found to be of no

material advantage and they occupied too much space at each end.

The reflecting spool is shown in detail in Fig. 2. It consists essentially of two disks connected by a short length of brass tubing. Each of these disks is composed of a piece of felt that fits the resonance tube closely, and each side of the felt is backed by a brass disk of diameter about 2 mm less than the diameter of the felt. This provides a good reflecting surface for the sound wave entering the tube.

A brass eye is fastened to the brass plate at each end of the reflecting spool, and to these eyes are attached the ends of the cord that controls the movement of the spool. The length of the cord is adjusted so as to have 1 or 2 cm of slack. This enables one to grasp the cord and move the spool with almost any desired speed; thus a very careful adjustment for the brass reflecting surface at the position of resonance of the vibrating air column is obtained.

It will be recalled that this is not the case with the apparatus in which the water reservoir is used to control the position of the reflecting water surface. There is very little flexibility of control of the position of the water surface. If the water is rising in the resonance tube toward the position for resonance, it may be stopped readily by means of some sort of pinch-clamp. If, however, it is necessary to set the water in motion, the reaction is sluggish and usually by the time the level has reached the desired height, the tuning fork is only faintly vibrating. In the present apparatus, the spool is moved backward and forward by the action of the hand and responds very readily to small accelerating forces in either direction.

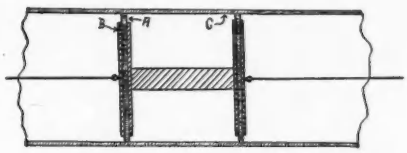

FIG. 2. *A*, felt disk; *B*, brass backing plate; *C*, resonance tube.

A student can readily perform the experiment alone when this resonance tube is used, whereas it is almost essential that two students work together when the usual glass tube with the water reservoir and rubber band is employed. Another very considerable advantage is the saving of the student's time. Using a fork of frequency 512 vib/sec, one can get five resonance lengths. The length is measured by thrusting a meter stick into the tube and against the brass disk while the cord is held tight in order to avoid any movement of the spool from the position of resonance. If each resonance length is taken as the mean value of five trials, then all the 25 lengths can readily be measured in 10 min. This is about the time required merely to set up the ordinary apparatus so that it is in working order.

THE specialized talents and abilities that are meeting this emergency and those that will meet emergencies to come are not produced by feverish last-minute activities. No amount of pressure can suddenly create a supply of thoroughly trained and broadly experienced physicists, mathematicians, chemists, biologists, economists and political scientists. These men represent the trained intelligence without which a war cannot be won, or a lasting peace achieved. They emerge spontaneously, unpredictably, but irresistibly out of long, patient and sustained effort. Pure research, the clean urge to gain new knowledge, the sympathetic appreciation of imaginative scholarship even when it seems remote and unrelated—these we must steadfastly sponsor or our vital intellectual resources will fail us in the days to come.—RAYMOND B. FOSDICK, *The Rockefeller Foundation, A Review for 1941.*

Direct Current Transients with the Square Wave Generator

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THE square wave generator which is used for amplifier fidelity measurements is well-adapted to direct current transient demonstrations. The technic is simple, the equipment is relatively inexpensive and the opportunities for analyzing any type of circuit appear unlimited. To those readers who are unfamiliar with this generator, one may say that it is a device for periodically applying and removing a direct voltage. There are various types of square wave generators—both electronic and mechanical. The electronic generator used by the authors required a sine wave control frequency which is supplied to the input. This wave has its upper and lower lobes clipped, amplified and reclipped. The output voltage wave is then, to all intents and purposes, square and of the same frequency as the input.

The output of the square wave generator is fed into the circuit under investigation, and the quantity under observation is applied to the plates of a cathode-ray oscillograph. The resulting transient phenomenon, which is repeated periodically, appears on the oscillograph screen as a continuing, or steady state, occurrence. While this technic is not new,[1] it deserves wider use because of its advantages. The transient is free from commutator ripples and other incident troubles. The image size is limited only by the size of the oscillograph screen, thus permitting exhibition to large groups as well as affording the opportunity of taking clear and sharply defined photographs. Another advantage of this technic is the opportunity it affords to observe transient changes while circuit adjustments are made.

The complete wiring diagram of the square wave generator and a circuit under investigation are shown in Fig. 1. The voltage drop across the resistance R', which is part of the circuit undergoing investigation, is applied to the cathode-ray oscillograph through the amplifier shown. The internal impedance of the generator is necessarily a part of the unknown circuit impedance. Its magnitude may be obtained readily. If it is desired to have a low internal impedance for the

[1] H. M. Turner, "Transient visualizer," Trans. A.I.E.E. 805 (1924); H. M. Turner, "Experimental method of studying transient phenomena," I.R.E. **19**, 268 (Feb. 1931); H. J. Reich, "Electronic transient visualizer," Trans. A.I.E.E. **55**, 1314 (1936).

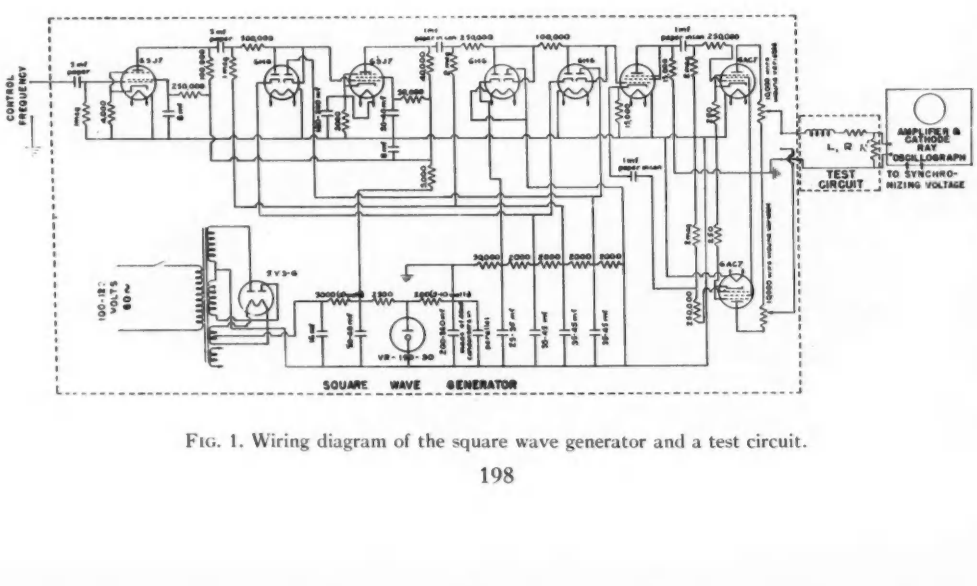


FIG. 1. Wiring diagram of the square wave generator and a test circuit.

FIG. 2. Growth and decay of the current in the circuit of Fig. 1.

generator, a simple series circuit resistive matching network can be used. This network must be regarded as an inherent part of the unknown circuit.

Figure 2 is a photograph of the growth and decay of current in the circuit shown in Fig. 1. The circuit parameters for this case are 2302 ohms resistance and 91.5 mh inductance at a frequency of 500 cycle/sec.

The question of how square is a square wave, is pertinent. For the generator designed and constructed at New York University, the base of the square wave was found to be 1.010 of the wave top at a frequency of 500 cycle/sec.

The series circuit with a total resistance of 1906 ohms and capacitance of 0.06 μf at a frequency of 500 cycle/sec gave the familiar oscillogram shown in Fig. 3. By applying the condenser voltage drop V_c to the oscillograph plates, the oscillogram of charge *versus* time was obtained, for $V_c = Q/C$. Since the current passing between the oscillograph plates is insignificant in comparison with the circuit current, the photograph obtained represents the growth of charge much more accurately than is possible with other technics. The upper curves in Fig. 3 represent current variation for this set of parameters. Those familiar with amplifier testing will recognize this oscillogram.

Figure 4 shows the current variation for a nonoscillating resistance-inductance-capacitance series circuit with parameters $R = 1971$ ohms, $L = 91.5$ mh and $C = 0.0010$ μf at a frequency of 7000 cycle/sec.

Figure 5 shows a mutually coupled circuit employed for the investigation of double energy transients, and Figs. 6 and 7 show the primary

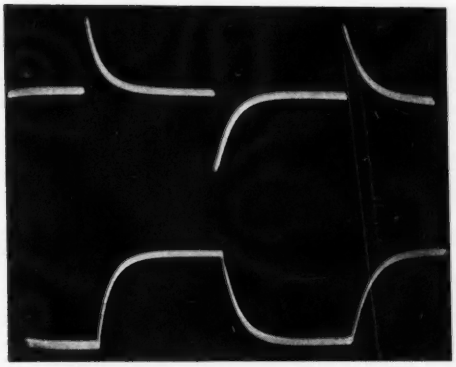


FIG. 3. Growth and decay of current and charge for a resistance-capacitance series circuit.

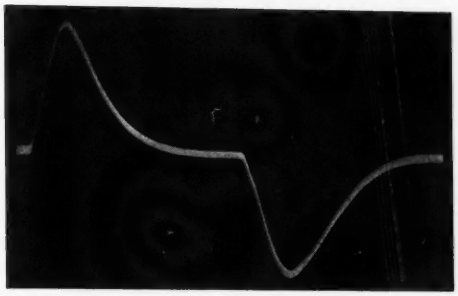


FIG. 4. Current variation for a nonoscillating resistance-inductance-capacitance series circuit.

and the secondary current, respectively. Figures 8 and 9 show, respectively, the variations of charges on C_1 and C_2, Fig. 5. It is interesting to observe that the current and the condenser charge variation of the secondary circuit are double energy transients. It is also quite interesting to observe the visual effects of changes of the circuit parameters on the wave form and oscillatory frequency.

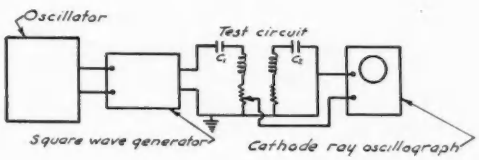
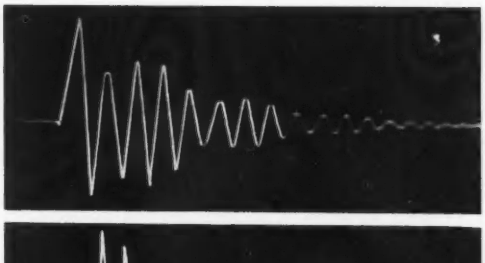
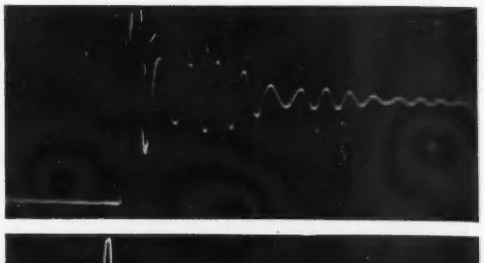


FIG. 5. Block diagram of the complete circuit for mutually coupled conditions.



FIGS. 6 and 7. Oscillograms of the primary (top) and secondary (bottom) currents of the circuit of Fig. 5.



FIGS. 8 and 9. Oscillograms of the charge variations across the condensers C_1 (top) and C_2 (bottom) of the circuit of Fig. 5.

An important feature of the square wave generator is that the driving frequency may be adjusted so as to obtain complete decay before the beginning of the next reversal. This is important where a desired condition must be fulfilled with nonadjustable circuit parameters.

The Navy V-1 and Army Enlisted Reserve Corps Plans

In a bulletin issued by the American Institute of Physics, under date of June 22, 1942, is published an exchange of correspondence between the Office of Scientific Personnel of the National Research Council and the Bureau of Naval Personnel of the Navy Department. The matter under discussion was the type of physics course that should be offered to students who are enrolled in the Navy's V-1 program.

It is part of the V-1 plan that an examination in several subjects—of which physics is one—will be given some time during the student's second year, probably about March 1. Since the results of this examination will determine the choice of men to be continued under plans V-5 or V-7, it is clearly important for physics departments to know what sort of course is best fitted to the needs of their V-1 students. From the information given by the Navy it appears that there will be no effort to dictate to the institutions but that it would be unwise for them to offer to V-1 students a course in physics that does not require laboratory work or that does not assume familiarity with trigonometry. It is pointed out, however, that only students with aptitude in the physical sciences and mathematics should be urged to *concentrate* in these fields, although it is advantageous to other V-1 students to take as much work in these subjects as their programs will permit.

One difficulty is apparent: students taking general physics during their second year will be confronted with an examination about March 1, before they have completed the course. Since many colleges are not able to require trigonometry for entrance, but must offer it in the first year, it may be desirable to begin the physics course during the summer session.

The War Department has announced its Enlisted Reserve Corps plan for college students, which is similar to the Navy V-1 plan. It contemplates the voluntary enlistment of students who will remain in an inactive status while they continue their education. In this plan, also, a qualifying examination will be given, probably during the latter part of the second year of study. It is understood that physics will be included in the examination only as one of several alternatives among the physical and social sciences. Students who are especially well qualified for advanced study may be recommended by their institutions to continue their studies beyond graduation.

Further information that should be of help in advising students with regard to enlistment in either of these programs is available in Bulletin No. 28, *Higher education and national defense*, American Council on Education, 744 Jackson Place, Washington, D. C.

NOTES AND DISCUSSION

Physics Training for the Negro Student

HERMAN BRANSON
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ON the basis of population, the Negro should contribute 10 percent of the physics students so urgently needed for the war emergency and for the scientific and technical work of the peace which will follow. It is true, however, that surprisingly few Negro students with the necessary basic abilities and aptitudes are thinking of physics as a career. Several factors are to blame. In the past the only application of physics open to the Negro has been in teaching. The difficulty and expense of preparing for a career in physics in comparison with one in, say, medicine or dentistry, where the opportunities for financial and social advancement have been greater, have been such as to send most of the able students into these other professions. Today there is great need for a concerted program to convince this 10 percent of our potential student population that physics is a valuable and applicable subject not only for the war emergency but in the preparation for research in other fields.

There are many able Negro students available for physics training. Howard University has been giving standard tests in a number of cities to select able secondary students for regional scholarships. From the returns on the rather small sample tested this year, we have the names of approximately 200 students whose superior abilities and expressed interests suggest that they could do a good job as physics majors. In addition to this secondary school group, there are others who have had scientific training in college but who are working at tasks not requiring their specialized abilities. When one also considers the large number of school science teachers whose physics needs bolstering and extending in order that they may better serve, we have some notion of the enormous problem facing the physics departments in Negro colleges at this time.

One approach to the problem would be to establish regional centers for systematic recruiting and training. The initial personnel and equipment for such centers will be found in colleges in or near key cities. The curriculum could be worked out in conferences. The entire program could well be established under ESMDT.

To help meet the immediate demand for physicists, Howard University has outlined a special one-year course aimed at retraining college graduates, out-of-school students and others who have had at least a year's course in general physics and mathematics through the calculus. By attending classes approximately 40 clock hours each week, it is thought that a select group of able young people enrolled in advanced general physics with laboratory, in electricity and magnetism with laboratory, in mathematical methods in physics, in mechanical drawing and shop practice, and in advanced laboratory—including acoustics, electronics and radio, heat, mechanics, optics and photography, and spectroscopy—will obtain the equivalent of a sound undergraduate major in physics. Those completing this program should be able to participate in minor research projects and developmental work. They would also constitute a reservoir of teachers for the much larger group of young people who will need basic physics and mathematics as prerequisites for technical training in the armed services. We hope that many students who complete the program will continue into graduate work, which will greatly extend their usefulness.

Baseballs *Do* Curve *and* Drop!

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University of Minnesota, Minneapolis, Minnesota

THE experiment recently described by Verwiebe,[1] together with the photographs of baseballs in flight which appeared in *Life*,[2] leaves no doubt about the lateral curvature of a baseball, despite *Life's* contention to the contrary. That a baseball must curve according to the laws of aerodynamics is clear; hence, the question is not, "Whether?" but, "How much?" The failure of a baseball to show the obvious curvature of path exhibited by a sliced golf ball in flight is attributable to the facts that the golf ball may be given a higher spin velocity and,

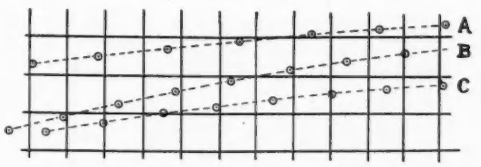

FIG. 1. Composite drawing of three baseball trajectories, as taken from Mili's flash photographs. Time element, 1/30 sec; squares, each 24 in.; diameter of baseball, 2.8 in. Measurement shows that the baseball paths were approximately four-fifths of the distance from the camera lens to the squared screen. *A*: "fast ball," 88 ft/sec; vertical acceleration *less* than 32 ft/sec². *B* and *C*: slower "curves," 69 ft/sec; vertical acceleration *exceeds* 38 ft/sec². Curves *B* and *C* exhibit almost the same horizontal velocity and vertical acceleration, but the distance of descent in *B* is some 16 in. greater than in *C* in the *same length of time* because the vertex of parabola *B* is closer to the pitcher than that of *C*. Curve *B* is a "drop," delivered from a higher elevation and with less initial upward component than *C*; the latter is a "sidearm curve," thrown from a lower level and with more upward velocity at the start. Only the last 18 ft of flight (*not* 22 ft) appear in each photograph.

more important, may be observed in flight for a longer time. Here again we meet our old friend, $F=ma$! During the flight of a pitched baseball, the linear speed is nearly constant, the spin velocity is likewise almost constant, and any Bernoulli forces introduced by the spin must cause a small constant acceleration of the ball at right angles to its path; but the time of flight is much less than 1 sec. The departure of the ball from its initial path is not large; but it must increase with the *square* of the time, so that three-fourths of the total deviation occurs during the last half of the flight and half of the deviation occurs

in the last three-tenths of the path from pitcher to batter. No wonder, then, that the ball *appears* to move sidewise faster and faster as it approaches the batter, for it *does* move faster and faster. Moreover, the angular velocity across the line of sight as viewed by batter or catcher increases still faster because of the rapid shortening of the radius vector to the ball.

But how about "drops"? Is the drop really more rapid than that for a ball thrown without spin? Yes, definitely! For the evidence, turn to the photographs by Gjon Mili[3] and apply the well-known "method of second differences" commonly employed in laboratory experiments on accelerated motions. The essential features of these pictures are reproduced in Fig. 1. Curve A shows a "fast ball," of horizontal velocity 88 ft/sec, whose vertical acceleration is definitely less than g; B and C are trajectories of slower "curves"—69 ft/sec—in which the vertical accelerations are nearly equal and are definitely greater than g. In fact, the difference in the accelerations of the fast ball and of the two slow ones is inescapable, indicating a departure of as much as 6 ft/sec^2 from g in the latter case. This is in accordance with the order of magnitude of acceleration indicated by Verwiebe's experiments[1] and suggested by the other photographs in *Life*, as well as by independent estimates made by the writer from observation of rotating cylinders of the same diameter as a baseball. A 200-gm mailing tube, 8 cm in diameter and 45 cm long, when spun about a horizontal axis and projected horizontally at moderate speed, may readily support its own weight or even rise, although its speed of progress through the air is much less than that of a baseball. Comparable forces acting on the lesser area of a baseball of mass 140 gm would cause it to accelerate at 5 or 6 ft/sec^2.

Therefore, it seems definite that a pitcher does not put spin on a baseball just for fun; he may cause the ball to veer *at least* 6 in. from its normal trajectory, up, down or sidewise, depending upon the direction of the spin axis, and in some cases the departures may be still greater. Now, admittedly, 6 in. in 60 ft is not much of a departure, but it makes the difference between a hit and strike in any ball game!

[1] F. L. Verwiebe, Am. J. Phys. **10**, 119 (1942).
[2] *Life*, Sept. 15, 1941, pp. 83–89.
[3] Reference 2, pp. 88–89.

A Large-Scale Demonstration of the Flight of Projectiles

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A DEVICE for showing the motion of projectiles on a large scale provides an impressive demonstration. Numerous devices of this kind have been proposed and are in use in many departments; the present note merely calls attention to a simple apparatus which we have used in this department for a number of years and which can easily be constructed from a piece of pipe, a spring, two wood blocks and an electromagnet. The path of the projectile is easy to see, and the fact that the acceleration of gravity acts independently of the horizontal component is impressively shown.

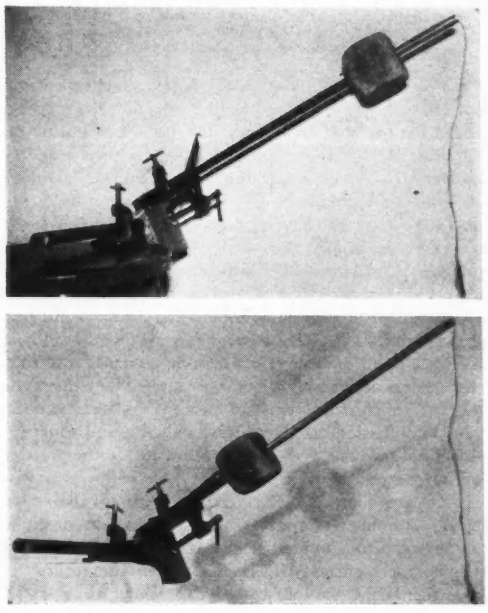

FIG. 1. Photographs of apparatus.

The apparatus can be used to demonstrate the law

$$\mathbf{r} = \mathbf{v}_0 t + \tfrac{1}{2}\mathbf{g}t^2,$$

where $\mathbf{r}$ is the position of the projectile at any time t, $\mathbf{v}_0$ is the initial velocity of the projectile and $\mathbf{g}$ is the acceleration due to gravity. By adding a protractor, stop watch and meter stick, or tape, one can also use this apparatus to demonstrate the simple relations between angle of elevation, maximum ordinate, range and time of flight. Such relations, however, apply only to motion in a vacuum, or to very slow motion in the atmosphere, and not to the flight of a military projectile.

The apparatus (Fig. 1) is extremely simple and can be built in any departmental shop. The barrel, a 3-ft length of ½-in. pipe, is clamped in an ordinary table clamp, and a fairly stiff spring about 16 in. long is slipped over the barrel and fastened at one end in the clamp under the barrel. Through the part of the clamp that would ordinarily fasten to the table is run a piece of 1-in. pipe about 1 ft long; it is held tightly in the clamp by two pieces of wood cut to fit snugly around the pipe. This pipe then fits into another clamp fastened to the lecture table. By loosening the upper screw of the second clamp the gun can be swung in a vertical plane and tightened in any position. By loosening the screw that holds the clamp to the table the gun can ordinarily be given enough horizontal swing

to line it up on the target. This double clamping arrangement is very strong, and there is little danger of the gun's vibrating if the table is rigid.

The gun is aimed by sighting through the barrel. The projectile is a 4-in. cube of very light wood with a hole through it so that it can slide over the barrel (Fig. 1). If heavy wood is used it may scar the floor or cause other damage. The trigger is a small lever which is fastened to the clamp that holds the spring and has a catch that engages an ordinary eye screw on one side of the projectile block. The falling target is a cube of wood similar to the projectile but having on one side a light piece of iron that can be attracted by an electromagnet. The electromagnet has enough turns so that a 6-volt battery can be used without a rheostat to excite it. The magnet has a handle by which it may be hung from a horizontal rod, thus facilitating speed of set-up. One connecting wire from the magnet has its tip end bent so that it can hang from the barrel and be knocked off as the projectile leaves the gun.

The few materials needed to make this apparatus are usually on hand in most science departments and very little time is required for constructing it. The main difficulty probably would be in finding a suitable spring.

The Time Concept in Restricted Relativity

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1. Professor Epstein's recent article[1] on "The time concept in restricted relativity" expresses with such admirable clearness a view of the special theory of relativity which I believe to be altogether mistaken, that it would be very regrettable if such an opportunity of clearing up a matter important for both the understanding and the teaching of relativity were allowed to pass unused.

Epstein starts from "the operational point of view," according to which "a physical concept is not completely defined until an instrumental operation is described which permits of reducing the concept to precise measurements." The adjective "instrumental" is perhaps misleading, since the operation often involves calculation as well, but in principle I agree completely with this starting point, and would indeed go further. I would say that a theory is not properly expressed until it is stated in terms directly applicable to the experiments which it is created to cover. A remote statement, in terms of an experiment which has never been carried out—that is, an operation which has not been performed—may or may not be demonstrably equivalent to the direct statement, but it cannot legitimately be made the foundation of an argument concerning the character of the theory unless that argument can be as validly based on the direct statement. It is just here that Epstein has abandoned "the operational point of view." He expresses the Lorentz transformation in terms of an imaginary comparison of observations by relatively moving observers, whereas experiment simply requires that a single observer must make this transformation if he changes his coordinate system. I will try to explain this in relation to the special theory of relativity.

The failure of the Michelson-Morley and other experiments to reveal the motion of the earth through the ether led to the special theory of relativity, namely: *It is impossible to detect the motion of matter relative to the ether;* or, in other words, *the absolute motion of a piece of matter.* This states the essence of the theory—no more and no less—directly in terms of the observations which generated it. It then became necessary, however, to explain why the experiments apparently offered a prospect of detecting the absolute motion of the earth. The answer was that this prospect arose from the use of false transformation formulas. The earth can be supposed at rest or moving round the sun, but if we change from one supposition to the other we must change the values assigned to the kinematical concepts used. The proper changes are given in general by the Lorentz formulas, which reduce in the Michelson-Morley experiment to the requirement that if a rod has length l when held to be at rest, it must be assigned length

$$l(1-v^2/c^2)^{\frac{1}{2}}$$

when held to be moving with velocity v in the direction of its length. The operation of measuring l is that familiar in physics, the measuring scale and rod being relatively at rest, but v is at one's choice since, absolute motion being undetectable, one can assign any desired velocity to a single piece of matter provided the observable relative velocities of other pieces of matter are not misrepresented. Thus, if the earth is regarded as being at rest, the ordinary expectation of no fringe-shift in the experiment is maintained; and if the sun is regarded as being at rest and the necessary change is made in the lengths involved, there is again no expectation of a fringe-shift. The experiment is thus explained in terms of a theory directly adapted to meet it.

Epstein, however, expresses the theory in terms of purely imaginary and practically impossible observations. He pictures two observers provided with identical yardsticks and clocks, who move with uniform velocity v with respect to each other. At a certain moment they meet in space, and at that instant they somehow succeed in assuring themselves that their clocks give exactly the same reading, after which they part forever. They observe external events, however, and some third party collects their records of these observations and compares them. He then discovers that they are related according to the Lorentz formulas.

Now I would not for the world deny that this would be so. I am so strongly attached to the operational point of view that I cannot do so without citing operations to justify my denial, and I cannot find them. But I cannot understand why Professor Epstein, who is apparently equally attached to the operational point of view, should express the theory of relativity in terms of imaginary operations instead of the actual ones. My perplexity is increased when I find that he goes on to draw conclusions from operations not merely imaginary but essentially

impossible. He attributes to me a "repeated insistence that the relativistic contraction of solid bodies is *not real* but due only to 'the mental attitude of the observer.'" (I cannot find that I used the words "not real"; I said rather that "no physical change" occurs in the bodies. I understand, however, very well what is meant by "real" here, so the point may pass, but in passing I cannot help finding it a little odd that Epstein should complain that "such a transcendental use of the word *real* is misleading for the beginner" when the word is his own interpolation and my phrase is anything but transcendental.) He then goes on to argue that the Lorentz contraction is "*real* in the ordinary sense of the word."

Well, however that may be, it is certainly not real in the *operational* sense of the word. To find if a rod has contracted we must compare its length now with its length before the contraction was supposed to have taken place, and this is of course an operational impossibility. From the operational point of view, therefore, the contraction, being unverifiable, is "unreal." I think Epstein's error arises from his rejection of the direct statement of the relativity theory referred to above. He goes on to say that "their 'mental attitude' is here of less account than their state of motion." But what is their state of motion but a consequence, in Epstein's phrase, of their "mental attitude"? The contraction is as "real" as the velocity which causes it, and since the theory of relativity allows us to assign to any single body whatever velocity—less than c—we please, it allows us to assign to it whatever contraction we please. True, the *relative* velocity of two bodies is "real," but the contraction arising from that may be assigned to whichever body we choose. Is an abstract "contraction," floating about in the ether and ready to attach itself to any body the observer may select for it, "*real* in the ordinary sense of the word"?

2. I pass now to the main subject of Professor Epstein's article, which is a criticism of the following argument which I set out in two articles in *Nature*.[2] I maintained that the whole modification of classical physics required by the special theory of relativity was contained in the "contraction" of lengths by the factor

$$\kappa = (1 - v^2/c^2)^{\frac{1}{2}},$$

all the other modifications—the Lorentz formulas, the "dilatation of time," the change of mass with velocity, and so forth—being logical consequences of this single change and the traditional definitions of physics. Consequently, the statement frequently made, that the rate of a moving clock is diminished by the factor κ, is not an independent postulate capable of being tested by experiment; if an experiment were made and the statement violated, the only legitimate inference would be that the clock when in motion did not indicate the time-scale of physics. I exemplified the last point by describing three types of clocks which, when at rest, would give identical time intervals between a given pair of events, but when in motion would give different intervals. I cannot do better than describe them here in Epstein's own words:[3]

> "The clocks considered by him are of the hourglass type: the elapsed time is measured by the amount of sand having run from an upper into a lower container. The quantity of sand in the lower container can be determined in several ways: (*a*) by measuring its volume, (*b*) by weighing it, (*c*) by counting the grains. In this way the hourglass principle gives rise to three different constructions which we shall differentiate by labeling them (*a*), (*b*) and (*c*).
>
> "As long as the three kinds of clocks are all at rest with respect to the observer, their scales can be adjusted so that they have the same rate and are perfectly equivalent because both the volume of the sand and its weight are proportional to the number of grains in it. It is otherwise when the clocks are given a velocity v with respect to the observer. The volume and weight are still proportional to the grain number, but the constants of proportionality are not the same as before. In fact, the theory of relativity shows that, in consequence of the motion, the volume of a given number of grains contracts in the proportion κ while their weight increases in the ratio $1/\kappa$. Therefore, the rates of the three moving clocks are not the same but stand in the proportions $\kappa : 1/\kappa : 1$, provided that their rates did not differ from one another when at rest."

Epstein's objection to my example is twofold. First, he maintains that only one of my three instruments is a "clock," namely, (*c*) (curiously enough, this is not the one that exhibits the true "time dilatation"; in Epstein's own words, "the two observers measure the same interval only if they count the same number of falling grains," in which case $\Delta t' = \Delta t$; this should have shown him that something was wrong with his argument, but let that pass) because a part of the *definition* of a clock is that its readings when set in motion must conform to the Lorentz transformation. "The question of the *legitimate* clock construction becomes quite simple," he writes; "It is identical with the question of the transformation of time units." The second aspect of his objection amounts to a charge that in (*a*) and (*b*) I have failed to indicate how the mass and volume of sand are indicated on the clock dial, and the charge is based on the contention that, in a true clock, the dial reading must indicate the time directly, without correction. "The theory [of relativity]," he writes, "simply insists that a moment of time is nothing but the reading of a clock"; and again, "*Every reading of a legitimate clock must be itself an event.* This condition is satisfied by any dial clock since the fact that the clock hand points to the number 12 . . .," and so forth. This shows clearly that it is the actual uncorrected dial reading which he insists upon as the true time. Let me examine these two points.

The first needs little comment: it is simply a matter of the meaning of words, and Epstein is clearly wrong. When I use the word "clock," I mean a time-measuring instrument which would be passed as accurate by the observatory at Greenwich or Washington; and that, I am sure, is the ordinary meaning of the word. If a clock has,

in addition, to satisfy the test that its readings directly exhibit the Lorentz transformation, then we do not know that a clock exists, for this test is never applied. Moreover, what is the point of saying that, according to the theory of relativity, "the rate of a moving clock is diminished by the factor κ" (the statement which the whole controversy is about) if an instrument is not accepted as a clock unless it satisfies this condition? The statement would then become a truism, and I say with confidence that neither the writers of the statement nor the numerous readers who have received it with surprise or incredulity regarded it as such. I therefore maintain my assertion that the "law of time dilatation" refers to the *time-scale* of physics, and is logically deducible from the law of "contraction" of lengths. What a particular clock will do when set in motion depends on the type of clock.

The second point of difference leaves me puzzled as to why a clock must necessarily give direct readings when no other physical instrument is required to do so. In an air thermometer, for instance, equal temperature steps are those corresponding to equal expansions of air at constant pressure, but the temperature on this scale is never equal to the observed reading even though the instrument is perfectly constructed and used. The expansion of the containing vessel must be allowed for, and corrections made for the exposed portion of gas and variations of pressure during the observations. Further, if we want the absolute temperature we must correct for the departure of air from the ideal gas state. All this is much more laborious than the simple reading of the mercury level, yet no one denies that an air thermometer is a thermometer. Or take the tangent galvanometer. The current is proportional to the magnetic field it creates at the center of the coil, but the pointer-reading must be corrected for errors of graduation, variations in the controlling field, deviation of the plane of the coil from the meridian, and so forth. Nevertheless, the instrument is a galvanometer. In these, as in all physical instruments, the quantity to be determined is defined by a *principle* which the instrument imperfectly embodies, and the physical quantity is obtained by correcting its readings. I see no reason, therefore, why the readings of a clock should not be corrected to conform to the *principle* of time measurement, which, as I showed in my articles, was, that equal times are those in which a beam of light covers equal spaces.

3. I should like to say before concluding how much I liked Professor Epstein's very clear explanation of the independence of an event of the system of coordinates chosen for describing it. I agree, of course, completely with this, but assumed that it was sufficiently well understood not to call for mention in brief articles. I cannot, therefore, at all understand Epstein's statement that "in the constructions (*a*) and (*b*), not only is the method of measurement individual with the observers (each measuring with his own instruments) but so also is the very interval which is measured." What does he mean by "the very interval which is measured"? I specified no interval. My clocks, like other clocks, were intended to measure time intervals between *any* pair of events, and I did not even instance specimen pairs. We can compare the readings of a pair of clocks—(*a*), (*b*) or (*c*)—one fixed and the other in a moving airplane, and determine from them the time interval between an explosion on the earth and the emergence of a satellite of Jupiter from the planet's shadow. If we could observe accurately enough we should then find that the comparison would be different for the three types of clocks. What is meant by saying that the interval is "individual with the observers"? It is the interval between objective events in each case. I suspect that, as with the word "real," Professor Epstein has been inventing an interval for me which has the characteristic he condemns, and has then blamed my clocks for it.

Of course, the determination of the times of the events involves some correction for the time of passage of light to the clocks, but that is necessary for every conceivable clock and so is irrelevant to the question of differences between clocks. Also, of course, every clock must have a dial of some kind, but it is immaterial what it shows so long as the method of correction is known. Indeed, a single instrument would serve for all three of my clocks, the correction being different in each case, just as a single instrument can be used as an ammeter or a voltmeter.

I see no reason, therefore, to doubt that the account of the special theory of relativity given in my little book is correct, and I have direct evidence that it is readily intelligible to students. I had already found from experience that they could understand and appreciate the value of the theory when so presented, and it was for that reason that I ventured to publish the treatment.

[1] Epstein, Am. J. Phys. **10**, 1 (1942).
[2] Nature **144**, 888 (1939); **146**, 391 (1940).
[3] Reference 1, p. 2.

The Time Concept in Restricted Relativity—A Rejoinder

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California Institute of Technology, Pasadena, California

1. Professor Dingle begins his reply[1] to my article[2] by burlesquing my description of the arrangement underlying the Lorentz transformation formulas. He refers to it as "purely imaginary and practically impossible observations" and goes on to say:

> He [Epstein] pictures two observers provided with identical yardsticks and clocks, who move with uniform velocity v with respect to each other. At a certain moment they meet in space, and at that instant they somehow succeed in assuring themselves that their clocks give exactly the same reading after which they part forever. They observe external events, however, and some third party collects their records of these observations and compares them. He then discovers that they are related according to the Lorentz formulas.

All this buffoonery is completely gratuitous. In the first place, they need not "succeed in assuring themselves that their clocks give the same readings" because they take the instant of their meeting as the zero point of their respective time scales *by definition*. In the second place, they must be in a state of uniform relative motion *only*

while they carry out their observations. Before and after they are free in their movements and have every opportunity in jointly preparing the experiment and jointly comparing their notes.

It seems that Dingle himself does not attach much weight to his objections, as he freely admits: "Now I would not for the world deny that this would be so." This admission, which negatives his charges, is very prudent, indeed, because to the extent to which observations of the sort described are essential for my article, far from being "purely imaginary and practically impossible," they not only could be tried but, actually, *have been successfully carried out*. In fact, the question with which we are here concerned is how the measured units of length and of time are affected by the state of motion of the measuring instruments. In this case, one of the observers can be dispensed with. Suppose that the observer A possesses a yardstick and a clock at rest with respect to him, while a second parallel yardstick and a second clock—identical with the first pair when both pairs are at rest—move past him with the velocity v. If there were another observer A', moving with the second clock and yardstick, it would be a matter of definition that he would regard the length of his (second) yardstick as 1 yd, and the period of revolution of his clock as 12 hr. Under these circumstances the services of the second observer A' are superfluous. The experiments consist in the observer A alone comparing the lengths of the two yardsticks and the periods of revolution of the two clocks.

As to the comparison of the lengths of the moving and the resting yardsticks, the experiment has not been carried out exactly in the described form. However, there is an overwhelming body of indirect evidence that the moving yardstick would be found the shorter by the factor $\kappa=(1-v^2/c^2)^{\frac{1}{2}}$, if the observations were accurate enough (Lorentz contraction). I may quote as my authority Professor Dingle, who wrote three years ago: "Circumstantial evidence for the effect described as a contraction of a moving rod is given by the Michelson-Morley and other experiments, and the observational justification for accepting it as verified is very strong."[3] Unfortunately he continued by saying: "There is no evidence of any kind for the definite retardation of clocks." This statement would have been considered as highly debatable for a good many years past as there existed strong indirect evidence for the retardation. It has been completely out of date ever since the publication of the beautiful work by Ives and Stilwell[4] on the transverse Doppler effect with which Dingle seems to be unfamiliar. The experiments of these authors constitute *direct* observations of the retardation of moving clocks—exactly as described above—the role of the clocks being played by the optical oscillations of atoms in a canal ray passing the slit of a spectroscope at a high speed ("atomic clocks").

In short, Dingle's objections against my starting point turn out to be altogether unfounded. Moreover, we shall explain in Sec. **3** why this starting point was particularly appropriate in view of the main purpose of the article.

2. The second objection of Dingle's refers to the reality or unreality of the Lorentz contraction. At first, I was greatly puzzled by it as I found myself confronted by the following cryptic statement: "To find if a rod has contracted we must compare its length now with its length before the contraction was supposed to have taken place, and this is of course an operational impossibility. From the operational point of view, therefore, the contraction, being unverifiable, is 'unreal'. . . . True, the relative velocity of two bodies is 'real'. . . ."

What other velocity but a relative velocity does there exist? The answer to this question is the solution of the whole mystery, which turns out to be not a problem of physics but of Professor Dingle's psychology. There was, in fact, a time in the history of physics when physical space was held to be filled by an all-pervading and immovable medium, the so-called "resting world ether." This hypothetical ether provided a frame of reference with respect to which an "absolute velocity" theoretically could be defined. Actually, however, every experiment designed to measure this absolute velocity had failed. The physicist of that period—about 35 years ago—was in the predicament that, on the one hand, he was supposed to be in motion with an absolute velocity with respect to the "world ether," while, on the other hand, he was forever prevented from knowing what that velocity was. It appears that this phase of development of physical science fell into Dingle's formative years and that its concepts are still in the back of his mind. At least, the passage just quoted and some other obscure passages in his writings make sense only when regarded from this point of view. The "length before the contraction" is apparently the length which the rod has when it is in the hypothetical state of absolute rest. Inasmuch as the observer cannot know what his absolute velocity is, he can attribute to himself any absolute velocity and thus his state of motion is said by Dingle to depend on his "state of mind" and to be "imaginary."

However, the difficulties of the old theory were long since resolved by Einstein and the "resting world ether" has been, for a long time, a thing of the past. From the modern point of view, Dingle's statement has no meaning whatsoever, since the state of (absolute) motion of an observer is not only arbitrary or "imaginary" but it is *nonexistent*, as there is no frame of reference with respect to which it could be defined. It is a term devoid of any meaning.

Naturally, his remarks with respect to my reality contention are based on a complete misunderstanding and have nothing to do with it. The contraction which I claim to be "real," is the same which was described in Sec. **1**. The observer A has before him two identical (when both are at rest) and parallel rods; the one is at rest with respect to A, the other is moving past him with the relative velocity v. We have seen that even Dingle admits that the moving rod is contracted. All that I claim is that the observer should not say that the rod only *appears* contracted. From the point of view of the observer, motion relative to him sets up in the rod additional molecular forces which he can calculate and measure. Therefore, imparting to the rod a (relative) velocity shortens it just as "really" as pressing it in a vise. In the meantime I happened to find out that in holding this opinion I am in good

company. I am indebted to Dr. R. A. Millikan for drawing my attention to the following passage by H. A. Lorentz:[5] "I should like to emphasize the fact that the variations of length caused by a translation are real phenomena, no less than, for instance, the variations that are produced by changes of temperature."

Incidentally, Dingle takes me to task for imputing to him the statement that the Lorentz contraction is "not real" while he had not used this word. The word which he had used was "metaphorical," and he made it quite clear in his reply that he certainly did not regard the contraction as real. I cannot see, under these circumstances, that there is enough difference in meaning between "metaphorical" and "not real" to justify his resentment.

3. The purpose of my article[2] was not to give a detailed criticism of Dingle's "Special theory of relativity" but rather to use it as an occasion for discussing a few special topics of an interest independent of Dingle's booklet. From the many statements in the book with which I disagree, I selected only two subjects about which I could contribute something in a general way helpful to the readers of the AMERICAN JOURNAL OF PHYSICS. One of these problems was the question of the "reality" of the Lorentz contraction referred to in the preceding section. But the larger part of the article was devoted to the other problem, the question of the "legitimate clock" in the theory of relativity.

Dingle advances the contention that it is impossible to conclude anything about the transformation of the rate of clocks in relativity, "for there is in physics no explicit definition of a clock." It is well to realize the possible implications of this statement. On the one hand, the theory of relativity does not recognize any other definition of an instant of time but as the (corrected and reduced) reading of a clock. On the other hand, its equations, especially the basic equations of the Lorentz transformation, contain the time as a variable. If there is no definition of a clock, then the concept of time itself is undefined and the relativists who use it in their formulas do not know what they are talking about. This is precisely the conclusion that Dingle draws.

It must be noted, however, that this conclusion is not inevitable even if his statement—"there is in physics no *explicit* definition of a clock"—is accepted as literally true. The fact is that the relativists, far from talking at random, knew very well what properties a clock must have in relativity; they had in mind an *implicit* clock definition. It is true, however, that while these properties were obvious to the physicists and mathematicians who used relativity as their tool, they had never been stated explicitly. The main purpose of my article was to supply this want; to give, for the benefit of the general reader, the definition of a *legitimate clock from the point of view of relativity* whose lack was pointed out by Dingle.

Professor Dingle begins his objections to this part of my article with a self-contradiction. Previously having insisted that there does not exist any definition of a clock, he suddenly declares in his reply that the definition is a matter of course: "I mean a time-measuring instrument which would be passed as accurate by the observatory at Greenwich or Washington." The criterions set up by the clock specialists of Greenwich and Washington certainly form an important part of the definition of the legitimate clock. But they are not quite sufficient, as these specialists do not put their clocks to relativistic uses and have no need to worry about the rates of timepieces moving with high velocities relative to one another. When these contingencies are taken into account, additional criterions must be introduced, namely, precisely those stated in my article.

Equally specious is the objection that I should not have based the discussion on the equations of the Lorentz transformation. The whole question is as to the meaning and the inner logic of these equations which comprehend, moreover, the larger part of the theory of relativity. What better approach to the solution of this problem can be found than the analysis of the equations themselves?

The account which Dingle pretends to give of my criterions for the legitimacy of a clock is a complete and fantastic misapprehension of them. He skims through my article and picks out a few sentences without bothering about their context; he imputes to me several assertions that I never made, at the same time completely ignoring the bulk and essence of my analysis. Statements which are falsely ascribed to me are as follows:

(1) That a legitimate clock must be a dial clock. What I said was that every dial clock is legitimate; there exist, however, plenty of clocks without dials which also satisfy all requirements. I mentioned specifically as legitimate two constructions of the hourglass type which were not dial clocks.

(2) That the time is determined only by an uncorrected reading of a clock. Made with such generality the statement is also a gross misconception. It goes without saying that the readings, by all means, should be corrected and reduced in agreement with the best standards of experimental technic. The only limitation on the theoretical corrections is that they should *not involve relativistic formulas*. When we are trying to test experimentally the validity of the relativistic transformations it would be begging the question if we applied these same transformations in our corrections. Such corrections are, of course, most stringently forbidden.

The actual criterions that I established were altogether different. I have the testimony of many readers of my article that they are there stated and justified with complete clarity. I regret, therefore, that I have to repeat them here, giving only a partial justification of them.

(1) The first condition is as to the type of *units* of the clock. The beginning and the end of a unit must be *events*, that is, instants defined independently of any frame of reference and, therefore, recognizable by any observer, whether he is at rest or in motion relative to the clock. The simplest example of such an event is the coincidence of the hand of the clock with a division of the dial. Hence, the dial clock was specifically mentioned as an illustration, giving to Dingle the false impression that the dial is a condition *sine qua non*. In this condition lies another cogent

reason why corrections based on relativistic formulas should not be applied to the clock readings. Such corrections can be made only from the point of view of a definite frame of reference and will render the corrected readings themselves dependent on this frame of reference, instead of their being *events* in the above-defined sense. This remark is pertinent in connection with Dingle's words: ". . . of course, every clock must have a dial of some kind, but it is immaterial what it shows so long as the method of correction is known." What he is thinking of is to provide his moving hourglass of construction (*a*) with a dial directly measuring the time in terms of the volume of the sand run out, as it (the volume) would appear to an observer moving with the clock. The stationary observer, past whom the clock moves with the velocity v, would observe these dial readings and correct them for the Lorentz contraction of the sand volume, from his own point of view. It is most palpable that this correction is illegitimate, as being based on the same Lorentzian equations which are here being tested, so that the resulting time unit is entirely arbitrary and not for any purpose comparable with the unit which the same observer would measure on an identical hourglass at rest with respect to him.

Professor Dingle pays me the compliment of approving of my "very clear explanation of the independence of an event on the system of coordinates chosen for describing it." It is strange that he completely misses the point for whose elucidation that explanation was designed and which, in effect, amounts to this: Without the relativistic correction (for contraction), the readings of the hourglass are *events* and can be used to define the time units of a legitimate clock; after the correction, *they are no longer events* and cannot be used so. This is also the explanation of my sentence, "in the constructions (*a*) and (*b*), not only is the method of measurement individual with the observers but also is the very interval which they measure," which provokes Professor Dingle's wrath, as he "cannot at all understand" it and he "does not specify any interval." Most sentences are hard to understand when they are torn out of their context. Perhaps Dingle did not specify any interval, but I most distinctly did. The interval whose measurement was discussed in the passages preceding the sentence quoted and to which the sentence itself refers is none other than the unit interval of a clock. We have already sufficiently shown that in Dingle's so-called clocks the time units are not intervals between *events*.

(2) The second criterion of a legitimate clock in the theory of relativity can be treated briefly as it is but little involved in the questions raised by Dingle. The resting clock is an instrument which measures the time coordinate alone, that is, the time at a given point in space. Hence the clock must be as point-like as possible in the frame of reference of the observer who moves with it and with respect to whom it is at rest. Always speaking from the point of view of this observer, this implies that the indicator of the clock must at the end of the time unit return to the same point at which it was at the beginning, or else, its failure to do so should be allowed for by an appropriate correction. Dingle's hourglass instruments do not violate this requirement and we have no reason to elaborate it any further.

Summarizing this section, I can say that, while storming at some imaginary charges of mine which, in reality, I had never preferred, Dingle ignores and fails to answer the actual objections which I did raise against his theory. As these objections are not drawn from thin air but are derived from an analysis of the fundamental equations of the theory of relativity, I am quite confident that they cannot be answered.

[1] H. Dingle, preceding article.
[2] P. S. Epstein, Am. J. Phys. **10**, 1 (1942).
[3] H. Dingle, Nature **144**, 888 (1939).
[4] H. E. Ives and G. R. Stilwell, J. Opt. Soc. Am. **28**, 215 (1938).
[5] H. A. Lorentz, *Problems of modern physics* (1927), p. 95.

Time of Collapse of a Soap Bubble

Inside a spherical soap bubble of radius r the excess of pressure over that outside is $4T/r$, where T is the coefficient of surface tension of the soap solution. If a bubble of radius, say, 8 cm is blown on the end of a glass tube of radius a and length l and is then allowed to deflate itself, the change in volume is given by the equation $-d(4\pi r^3/3) = (\pi a^4/8\eta l)(4T/r)\mathrm{d}t$, where η is the coefficient of viscosity of the air and streamline flow is assumed. This equation becomes

$$-\int_R^{\,} r^3 \mathrm{d}r = \int_0^{\tau} (Ta^4/8\eta l)\mathrm{d}t;$$

whence $R^4/\tau = Ta^4/2\eta l$, where R is the initial radius of the bubble and τ is the time of collapse. Thus a method of estimating the surface tension coefficients of different soap solutions is suggested. Under suitable conditions, a determination of the viscosities of certain gases *or* their effect on the surface tension of soap films is also rendered possible.—L. Sibaiya, *Nature* **149**, 527 (1942).—J.D.E.

Proceedings of the American Association of Physics Teachers

The Meeting at the Pennsylvania State College, June 25–26, 1942

THE American Association of Physics Teachers met at The Pennsylvania State College on June 25–27, 1942, jointly with the Society for the Promotion of Engineering Education and concurrently with the American Physical Society. A joint dinner of the three societies was held on Friday evening, June 26, in the Nittany Lion Inn. The program for the meeting was arranged by L. W. Taylor, representing the Association, and James G. Potter, representing the S.P.E.E.

Invited Papers

On Thursday, June 25, the Association joined with the Society for the Promotion of Engineering Education in an all-day symposium on "The war and problems in physics teaching." The following invited addresses were heard:

The special war training program in physics at Brown University. R. B. Lindsay, *Brown University, Providence, Rhode Island.*

The ESMDT foundations-of-engineering course offered by the Extension Service of the Pennsylvania State College. Marsh W. White, *The Pennsylvania State College, State College, Pennsylvania.*

Some problems of the ESMDT institutional representative. C. A. McKeeman, *Case School of Applied Science, Cleveland, Ohio.*

Some aspects of the ESMDT program. I. H. Solt, *U. S. Office of Education, Washington, D. C.*

Education in physics for the war program. H. L. Dodge, *University of Oklahoma, Norman, Oklahoma.*

The program for retraining of teachers in high school physics. M. H. Trytten, *University of Pittsburgh, Pittsburgh, Pennsylvania.*

Training men in acoustics and supersonics for war research. Leonard Olsen, *Case School of Applied Science, Cleveland, Ohio.*

Optics in the war effort. Brian O'Brien, *University of Rochester, Rochester, New York.*

The research physicist—his characteristics and training. Ross Gunn, *U. S. Naval Research Laboratory, Washington, D. C.*

Research in physics for the war program. K. T. Compton, *Massachusetts Institute of Technology, Cambridge, Massachusetts.*

Contributed Papers

The session on Friday morning, June 26, was devoted to the following contributed papers:

1. War gives new meaning to physics courses for college women. Ann Timberlake, *Mary Baldwin College, Staunton, Virginia.*—For the abstract, see the program of the Southeastern Section of the Physical Society, in this issue.

2. Physics training for the Negro student. Herman Branson, *Howard University, Washington, D. C.* (By title.)—The complete paper appears under "Notes and Discussion," in this issue.

3. The basic optical formula. C. R. Fountain, *Amherst College, Amherst, Massachusetts.*—The formula, $(n_1/p)+(n_2/q)=(n_2-n_1)/r$, applies to all cases of light crossing a boundary of radius r between two mediums of refractive indexes n_1 and n_2, respectively. We only need to adopt the convention that all distances in the direction of propagation of the light are positive, including the distances from the boundary to the center of curvature, principal focus and image. The image formed in one medium is considered as the object for the next medium, but the distance to it is negative if it is beyond that boundary. Application of this formula successively to the various boundaries enables one to solve problems with all kinds and combinations of lenses, thick or thin, and with any spacing along the optical axis. Principal points, principal planes and nodal points are eliminated. Principal foci are needed only in making scale drawings for thick lenses or combinations thereof.

4. Stimulating student reliance on comprehending in preference to memorizing. James G. Potter, *South Dakota State School of Mines, Rapid City, South Dakota.*—A major problem which does not appear to receive adequate recognition in the deliberations of teachers of beginning courses is that of breaking down an innate unwillingness of most students to rely on their abilities to reason from fundamentals. As a result of earlier school experiences, students are inclined to rely on memory, often using it as a means of avoiding the necessity for comprehension. Probably the foremost function of the physics teacher is to instil in the student a self-confidence in his ability to recognize in a situation the operation of the fundamental principles and then to deal with these principles, using the mathematical tools at his disposal, to obtain significant results. When the student has satisfied himself of his ability to reason from fundamentals in a few situations, his appetite for additional basic concepts is greatly improved. The practice of textbook writers in presenting extended algebraic treatments of a concept before demanding that the student reason from the concept by such methods as he can readily conjure, tends to foster memorization of formulas instead of the acquisition of power to apply concepts to new situations. When a concept that may be deduced from previously mastered concepts is not introduced in the textbook from a point of view which makes this apparent, the student is deprived of the opportunity to rely on his comprehension instead of his memory.

5. The use of calculus in the physics course for engineering students. J. W. Woodrow, *Iowa State College, Ames, Iowa.*—For several years there has been a growing conviction among members of our physics staff that a closer correlation should be developed between the courses in elementary mathematics and in physics for engineers.

Conferences between members of the mathematics, engineering and physics staffs resulted in a regulation, heartily endorsed by the Dean of Engineering, that requires the simultaneous classification of engineering students in calculus and general physics. This rule has been in effect for two years, and the results have been very encouraging from the standpoint of both the mathematics and physics departments as well as the engineering division. Furthermore, the students enjoy the work more and say that they feel that they have a better foundation for their future engineering courses. Methods, course content and results were discussed.

The Meeting at the University of Utah, June 16, 1942

THE fifth meeting of the American Association of Physics Teachers in affiliation with the Pacific Coast Division, American Association for the Advancement of Science, was held at the University of Utah, Salt Lake City, on June 16, 1942. The program was arranged by a regional committee headed by Orin Tugman.

The following papers were heard:

Correlations of force and field intensity in gravitational, electric and magnetic fields. LYNN W. JONES, *University of Redlands, Redlands, California.*

Physics and agriculture. WILLARD GARDNER, *Utah State Agriculture College, Logan, Utah.*

The significance of the increased demand for physicists. WAYNE B. HALES, *Brigham Young University, Provo, Utah.*

The session ended with a general discussion of these papers.

Report of the Association Representatives before the American Council on Education

THE twenty-fifth annual meeting of the American Council on Education, held at Chicago, Illinois, May 1 and 2, 1942, marks the third meeting in which the American Association of Physics Teachers has participated since it joined the Council. It is gratifying to report that the ground prepared in two previous years has begun to bear fruit and that certain steps of importance to physics teaching and to science teaching in general were instituted as a result of conferences held during the meetings. The meeting this year was naturally exercised over the military needs of the country and the responsibility of educational institutions to help meet these needs as effectively as possible. Physics and mathematics were mentioned repeatedly as involving fundamental skills which are needed in the training of men for the armed forces; there are numerous evidences that the demand for men with such skills is constantly increasing and that the demand for competent instructors in physics and mathematics will outstrip anything imagined before the outbreak of the war. The importance and the difficulty of keeping able teachers on the job is becoming increasingly evident, and for improvement in the present situation some look hopefully toward the newly formed federal manpower commission.

At a breakfast table conference inaugurated by the Association representatives, a small group of men from the Council met on May 2 to discuss two important concerns: the immediate and pressing need for teachers of physics and mathematics, and the program of cooperative effort toward the improvement of teacher training and coordination of curriculum in secondary school science which was inaugurated by the Association in 1940 and has been making rapid progress. Present at this conference were: George F. Zook, President of the American Council on Education; Karl W. Bigelow, Director of the Council's Commission on Teacher Training; R. J. Havighurst, Chairman of the Cooperative Committee on Science Teaching; Roscoe L. West, President of the American Association of Teachers Colleges; Harry A. Sprague, President of New Jersey State Teachers College, Upper Montclair, New Jersey; Frank B. Knight, Director of the Division of Education and Applied Psychology, Purdue University; P. E. Klopsteg, Chairman of the Governing Board of the American Institute of Physics and Treasurer of the Association; and the three representatives whose names appear below. At least six of these ten men had an intimate acquaintance with the previously reported work of the Association Committee[1] and of the Cooperative Committee on Science Teaching.[2]

This meeting afforded opportunity to lay before influential persons in the Council the progress of our work and to enlist certain definite support. For the present, this support will consist chiefly in making the program more widely known and understood through publications of the Council, and in probable financial support of two fellowships for work on a coordinated science curriculum for secondary schools, such work to be undertaken in Indiana where further assistance will be forthcoming from Purdue University and where a sample cooperative program is already developing beyond the stage of committee generalities.

The Association representatives also conferred with John W. Studebaker, United States Commissioner of Education, on the prospects of immediate action aimed to inaugurate supplemental training for secondary school teachers of science and mathematics in order to cultivate a promising source of manpower for the training of the large personnel needed in the armed forces and the defense industries. This has been a matter of some concern to the officers and certain committees of the Association for several months.[3]

The United States Office of Education serves as a liaison agency between defense and educational groups, and it is anticipated that prompt action may be instituted toward the training of a large corps of teachers to meet the present need.

Although a number of the general sessions of the Council were of interest to teachers of physics, only brief comments on two or three major points are possible here. Floyd W. Reeves, Director of the American Youth Commission of the Council, pointed out the likely continuation of the drop in secondary school and college enrolments in the near future and also the increased emphasis upon technological training, for which those who teach physics and mathematics will be particularly in demand. He further pointed out the need to plan now for post-war demobilization; this, he anticipated, would be a slow process during which large numbers of men would be continued in our colleges on Federal subsidy. Representatives of both the Army and the Navy stressed the importance of keeping young men at their studies until college graduation so as to provide a steady flow of trained manpower; they looked upon unrestricted enlistment as a shortsighted expedient. These are the days when men who know something about mechanics, electronics and trigonometry are greatly in demand, and one cannot escape the conclusion that those who train young men in these fields are doing work of utmost national importance. Nor can one escape the conclusion that the job is going to be bigger than ever for many months to come.

C. J. LAPP
K. LARK-HOROVITZ
May 5, 1942 R. M. SUTTON

[1] K. Lark-Horovitz, Am. J. Phys. **10**, 60 (1942); reprinted in Rev. Sci. Inst. **13**, 137 (1942). Also see R. J. Stephenson and G. W. Warner, Am. J. Phys. **9**, 50 (1941); M. H. Trytten and J. M. Leach, Am. J. Phys. **9**, 96 (1941); and E. H. Warner, Am. J. Phys. **9**, 368 (1941).
[2] G. W. Warner, Am. J. Phys. **10**, 121 (1942).
[3] See, for example, Am. J. Phys. **10**, 126 (1942).

RECENT MEETINGS

ASSOCIATION OF PHYSICS TEACHERS OF WESTERN PENNSYLVANIA AND ENVIRONS

THE Western Pennsylvania and Environs chapter of the American Association of Physics Teachers met at Westminster College on May 9, 1942. Approximately 40 members and guests attended. The morning session was devoted to the following twelve-minute papers:

The symbol π and its implied significance in physics. BERNARD L. BRINKER, *St. Vincent College.*

Nonpriority equipment. R. C. HITCHCOCK, *Indiana State Teachers College.*

Demonstrations of some new equipment. JOHN G. MOORHEAD, *Westminster College.*

Terminology in thermodynamics. W. W. MCCORMICK, *Geneva College.*

What is mass? W. H. MICHENER, *Carnegie Institute of Technology.*

Determination of the refractive index of a liquid by means of the lens-maker's equation. GLENN Q. LELFER, *Kent University.*

A simple determination of e/m. JOSEPH LEPIRE, *Allegheny College. (Introduced by C. W. Ufford.)*

The solution of equations by substitution in formulas. GEORGE E. DAVIS, *Duquesne University.*

Some energy interchange within a gas. A. G. WORTHING, *University of Pittsburgh.*

Following a luncheon, Wheeler P. Davey, of Pennsylvania State College, led a round-table discussion on "The proposed basis of self-rating by physics departments of American colleges."—O. H. BLACKWOOD, *Secretary.*

OREGON CHAPTER OF THE ASSOCIATION

The thirty-first meeting of the Oregon Chapter of the American Association of Physics Teachers was held at Oregon State College on April 25, 1942. The morning meeting was held jointly with the Student Science Conference and was devoted to four papers of general interest to scientists. This session was followed by a business meeting at which Will V. Norris, Chapter President, presided. The following officers were elected for the year 1942–1943: *President*, W. R. VARNER, Oregon State College; *Secretary-Treasurer*, E. H. COLLINS, University of Oregon.

At the luncheon and afternoon sessions, three papers were heard:

War psychology. O. R. CHAMBERS, *Oregon State College.*

Discussion and explanation of the ESMDT program. A. A. KNOWLTON, *Reed College.*

A demonstration with 10-cm waves. E. A. YUNKER, *Oregon State College.*

The Secretary of the Chapter discussed certain aspects of the work of the Association and also the progress of the ESMDT program.—W. R. VARNER, *Secretary.*

SOUTHEASTERN SECTION OF THE PHYSICAL SOCIETY

The eighth annual meeting of the Southeastern Section of the American Physical Society was held at the University of Mississippi, Oxford, on April 10 and 11, 1942. Approximately 100 members and guests attended. Local arrangements were made by a committee headed by W. L. Kennon.

The regular program consisted of 22 papers. Abstracts for five papers relating to physics instruction appear below. Abstracts of the remaining papers appear in the *Physical Review*, **61**, 725–729 (1942). Other features of the program were a talk by J. C. Morris on "The part of the physicist in the defense program," a conference of department heads on current problems, and a general conference on defense courses. The Section also participated in the formal opening of the University of Mississippi Observatory, at which D. V. Guthrie spoke on "Celestial laboratories."

At the business meeting the election of the following officers for the year 1942–1943 was announced: *Chairman*, W. L. KENNON; *Vice Chairman*, L. L. HENDREN; *Secretary*, E. SCOTT BARR; *Treasurer*, C. B. CRAWLEY; *Member of the*

Executive Committee, A. W. DICUS. The invitation extended by the Alabama Polytechnic Institute to hold the next meeting at Auburn, Alabama, was accepted.—E. SCOTT BARR, *Secretary*.

CONTRIBUTED PAPERS

1. The war gives new meaning to physics courses for college women. ANN TIMBERLAKE, *Mary Baldwin College, Staunton, Virginia*.—Recently several syndicated newspaper articles have treated the problem of the role of women in business and industry during and after the war. In one of these, the women's colleges have been severely criticized for offering almost no courses that prepare the student to earn a living. Although the British have used women extensively to replace men as machine operators, a large proportion of the men called to war service from industry have held more technical positions. That properly trained American women are not being supplied in sufficiently large numbers to replace such men is indicated by the fact that, of the physicists listed in *American men of science*, only 3.1 percent are women [Am. J. Phys. **10**, 52 (1942)]. The problem of replacing men need not be solved by turning the women's colleges into trade schools. Thorough training in the fundamentals of physics and other sciences would not only meet our immediate need but would provide a large group prepared to use the methods of the scientist in attacking postwar problems. Enrolment in these courses in women's colleges may be increased by the substitution of required courses in science for the present elective ones, devoting more effort to interesting the students, providing more extensive training in mathematics, and increasing the emphasis on laboratory equipment and teaching personnel.

2. Efficiency of instruction in college physics. CHARLES W. EDWARDS, *Duke University, Durham, North Carolina*.—The ratio of output to input, we teach, is an important factor in estimating the value of a machine. The modern industrialist would quickly discard a machine that converts a major part of the input energy into useless heat. Is a college administrator equally ready to junk educational machinery that develops more heat than light? Is he interested in appraising as accurately as possible the concrete useful output of his plant and is he interested in objective, experimental methods of arriving at the truth? It may be worth while for an instructor to attempt to determine how many topics he has presented to his class in hours of patient demonstration and exposition which never registered at all with his students. This paper described objective methods that have been employed in determining the useful "output" of physics lectures, demonstrations, recitations and individual laboratory work and of the "carry over" into schools of medicine and engineering.

3. Lecture demonstrations of luminescence. CHARLES W. EDWARDS, *Duke University, Durham, North Carolina*.—A series of demonstration experiments in phosphorescence and fluorescence with materials which have recently become available were described. The interesting chemiluminescence of 3-aminopthalhydrazide was demonstrated.

4. Direct applications of physics laboratory equipment on aircraft. SHERWOOD GITHENS, JR., *Air Corps Technical School, Sheppard Field, Texas*. (By title.)—The completeness with which the principles of physics are applied in the various features of the modern military airplane is remarkable. The ingenious, direct application of several devices, usually looked upon simply as equipment for physics laboratory experiments, was discussed.

5. Training methods in science in the Air Corps technical schools. SHERWOOD GITHENS, JR., *Air Corps Technical School, Sheppard Field, Texas*. (By title.)—The Air Corps schools for aviation mechanics are interested in speedily training a large number of men in the inspection and preventive maintenance, rather than the repair, of aircraft. The "line mechanic" must be able to detect symptoms of correct or faulty operations, to predict troubles, and to analyze the source and nature of a difficulty when it arises. Hence, the objective is largely training of the mind rather than of the hands. The task is to teach, in the time available, sufficient physics to grasp the operating principles of the various aircraft mechanisms. Some of the features of the training program represent interesting applications of pedagogic principles and interesting contrasts to conventional methods of science teaching.

You cannot do without one specialty. You must have some base line to measure the work and attainments of others.—LOUIS AGASSIZ.

RECENT PUBLICATIONS

GENERAL COLLEGE PHYSICS

College physics. A. WILMER DUFF, Emeritus Professor of Physics, and MORTON MASIUS, Professor of Physics, Worcester Polytechnic Institute. 598 p., many illustrations, 27 tables, 14×22 cm. *Longmans Green*, $3.80. PROFESSOR DUFF's *College physics*, well known among teachers as "The little Duff," was first published in 1925 and revised in 1926 and again in 1935 [Am. J. Phys. 4, 146 (1936)]. With the collaboration of PROFESSOR MASIUS, it has now been entirely rewritten in an enlarged form, with improvements in presentation and modernization of the material. As in the earlier editions, fundamental principles are stressed; no attempt has been made to include details of engineering applications of physics. The few brief proofs involving the calculus which formerly appeared in the text proper have been placed in an appendix. All of the many problems are new.

College physics. Ed. 3. WILLIAM T. MCNIFF, Assistant Professor of Physics, Fordham University. 679 p., 262 fig., many tables, 16×24 cm. *Fordham Univ. Press*, $4. The 1935 edition of this general textbook for science, premedical and pre-engineering students has now been revised and augmented and, moreover, the former two volumes have been combined into one. As before, the treatment is straightforward, with no attempt at cajolery, and is at least average in grade of difficulty; but it is also clearly and thoughtfully developed. There are seven parts—on mechanics, heat, sound, light, magnetism, electricity and modern physics, in the order named. The sequence in mechanics is somewhat novel in that it begins with fluids, elasticity and viscosity, passes to acceleration, force and the concepts of simple vibratory motion, and concludes with machines, work and momentum. Two of the 49 chapters are devoted to medical applications. There are many worked examples and problems for student solution. The typography is excellent.

Introduction to physics. HARLEY HOWE, Professor of Physics, Cornell University. 571 p., many figures, 18×27 cm. *McGraw-Hill*, $3.75. The present textbook has been developed primarily to meet the needs of the large group of liberal arts, premedical and agricultural students who enter the general course with little or no precollege physics and no mathematics beyond school algebra and plane geometry. Such students are expected to receive both practical and cultural benefits from a course in physics, and it has been the purpose of the author to afford them these benefits without reducing the course to "trade-school" physics or to a mere survey of "what physics is about." The book obviously has been planned with great care; it is characterized by clarity of language, discriminate use of terminology and continuity of treatment—"the 45 chapters . . . tell one story and not 45." In mechanics, which starts from Newton's laws, only the cgs absolute and the fps gravitational systems of units are stressed. In the treatment of electricity, which begins the second half of the book, magnetism is made incidental to currents. The practical units in electricity are treated as convenient multiples of cgs units rather than as derived mks units. Lists of questions and problems accompany each chapter.

INTERMEDIATE AND ADVANCED PHYSICS

Temperature measurement and control. ROBERT L. WEBER, School of Chemistry and Physics, The Pennsylvania State College. 440 p., 183 fig., 15×22 cm. *Blakiston*, $4. This is a printed and clothbound edition, somewhat revised, of a book on the theory and modern practice of temperature measurements which appeared originally in 1941 in a lithoprinted form [Am. J. Phys. 9, 195 (1941)]. It provides a textbook and laboratory manual of 24 experiments for use in a course for junior students of engineering and science who have had elementary calculus and a year of general physics.

Acoustics of buildings. Ed. 3. F. R. WATSON, Professor of Physics, Emeritus, University of Illinois. 181 p., many illustrations, 15×23 cm. *Wiley*, $3. The present edition of this textbook has involved a rewriting of the text to incorporate the developments in the acoustics of rooms and in sound insulation that have occurred since the appearance of the 1930 edition. Account is taken of the recent advances in the theory of vibrations of small rooms which have been effected through a study of "normal modes" of vibration, of the resulting new formulas which apply particularly to small rooms, of the reconciliation of experimental disagreements in absorption coefficients through measurements of acoustic impedance, of the extensive new tables of absorption coefficients and data related to soundproof construction, and of the preliminary attempts now in progress to specify acoustic standards for auditoriums and for the performance of sound-amplifying systems. Another feature of the present edition is the introduction of numerous problems for student solution.

A laboratory manual of electricity and magnetism. Rev. ed. LEONARD B. LOEB, Professor of Physics, University of California. Approx. 350 p., 34 fig., 15×23 cm. *Stanford Univ. Press*, paper cover, $1.90. In developing a laboratory manual to accompany his *Fundamentals of electricity and magnetism*, the author has endeavored to set up for each experiment a clearly defined objective which is closely coordinated with the textbook and designed to emphasize by direct observational study only the most basic of the material treated in the text. The experiments are strictly quantitative and, so far as possible, involve methods and technics that are in common use in electrical measurements. The author points out that he has chosen to follow the procedure developed in Millikan's *Mechanics, molecular physics and heat*—that of prefacing each experiment with a relatively complete and self-contained theoretical discussion.

The 12 experiments deal with the plotting of a magnetic field, absolute evaluation of a given field and a test of the inverse square law, tangent galvanometer and the laws of electrolysis, measurement of potential difference in emu, Wheatstone bridge, potentiometer, moving coil galvanometer, absolute measurement of capacitance, mapping of an electric field, magnetization curve for iron, capacitance by comparison with a standard, and coefficients of self- and mutual induction. Elaborate printed forms for recording data and computations are provided.

A textbook in electricity and magnetism. HARRY C. KELLY, Associate Professor of Physics, Montana State College. 363 p., many figures, 15×23 cm. *Wiley*, $3.75. This textbook is intended for an intermediate course in electricity and magnetism for science and engineering students who have had college mechanics and heat and who are at least enrolled in integral calculus. The method of approach is through mechanics, not merely to show the analogy in the type of reasoning but to elucidate the idea that the study of electricity was undertaken by men whose familiarity with mechanics naturally led them to adopt the tools that had proved most useful and convenient. There are 28 relatively brief chapters, each supplied with many problems. The treatment begins with a review of essential mechanical concepts, including those of the gravitational field and potential, and of simple cases of line integrals. Electrostatics is followed by a consideration of moving charges in a vacuum and in resisting mediums. Ohm's and Kirchhoff's laws are presented as outgrowths of the force equation for an electron. After a discussion of magnetostatics, the law of induction is obtained from a consideration of the relative motion of a magnetic field and an electron. Several chapters are devoted to alternating currents. Finally, the Maxwell equations are introduced for the analysis of the radiation resulting from the acceleration of a charge. The author has refrained from attempting to make his treatment encyclopedic. The modern material included is considerable but not disproportionate in amount.

Statistical thermodynamics. R. H. FOWLER, Fellow of Trinity College and John Humphrey Plummer Professor of Mathematical Physics, University of Cambridge, and E. A. GUGGENHEIM, Lecturer in Chemical Thermodynamics, Imperial College of Science and Technology, University of London. 703 p., 18×27 cm. *Cambridge Univ. Press* and *Macmillan*, $9.50. As the authors point out in the preface, the old kinetic theory of gases developed by CLAUSIUS, MAXWELL and BOLTZMANN has now been expanded so widely and deeply that a more adequate name for it is the *atomic theory of the properties of matter in bulk*. Moreover, in this expansion the theory has separated into two branches: *the theory of the properties of matter in equilibrium*, which is commonly, although perhaps inaccurately, called "statistical mechanics," and *the theory of transport phenomena*. The present treatise is concerned mainly with the first of these branches; in fact, it is a modified version of the senior author's recently revised *Statistical mechanics*. But it is a version that has been prepared expressly for the use of students of physics and chemistry, and hence much of the material of purely astrophysical interest and many of the mathematical details of proofs are omitted. On the other hand, much more emphasis than heretofore is placed on the parallelism of statistical mechanics and thermodynamics so as to provide as much of a link between these fields as is possible; hence the title of the book, *Statistical thermodynamics*. The main topics treated are the fundamental assumptions of statistical mechanics, general theorems for assemblies of permanent systems, permanent ideal gases, crystals, chemical equilibriums and evaporation, grand partition functions, non-ideal gases, liquids and solutions, surface layers, electron theory of metals, lattice imperfections, electric and magnetic properties. Although chemical kinetics properly falls in the theory of transport phenomena, a chapter on this subject is included because of its interest to the students.

MATHEMATICS

Higher mathematics for engineers and physicists. IVAN SOKOLNIKOFF, Associate Professor of Mathematics, AND ELIZABETH S. SOKOLNIKOFF, formerly Instructor in Mathematics, University of Wisconsin. 598 p., 157 fig., 15×23 cm. *McGraw-Hill*, $4.50. Much of the material of the 1934 edition of this textbook has now been rearranged and supplemented by new illustrative examples, and the number of problems has been more than doubled. A new chapter on complex variables incorporates some of the material that formerly appeared in the chapter on conformal representation; and the former chapter on elliptic integrals is now included in much enlarged chapters on infinite series and differential equations. The one-year course for which the book is designed might well replace the usual courses in advanced calculus and differential equations for students of engineering and applied physics. The authors have so chosen their illustrative material that both underlying principles and the practical utility of mathematics are nicely emphasized.

A short course in tensor analysis for electrical engineers. GABRIEL KRON, Consulting Engineer, General Electric Company. 265 p., 146 fig., 15×23 cm. *Wiley*, $4.50. Based on a series of lectures delivered to students in the Advanced Course in Engineering of the General Electric Company, this textbook on applied tensor analysis deals in the first part with the invariant theory of general asymmetric networks and, in the second, undertakes a more detailed analysis of one special type of such networks, namely, that represented by rotating machines. The author remarks that tensor analysis has hitherto been used exclusively to establish the invariant laws of nature in the form of tensor equations which are independent of the reference frame employed and that little attempt has been made to expand these symbolic equations to particular cases; that in the present book, on the contrary, the establishment of the symbolic equations is merely a step toward the final goal of developing a routine method by which the relatively few symbolic equations may be applied to the infinite variety of specific problems encountered in modern engineering.

Although electrical applications of interest to the power engineer are used in the book, the method developed may be applied equally well to mechanical systems. It is of interest that the author has made every effort to employ in this engineering textbook the absolute minimum of concepts and only those that form the very foundation of theoretical physics.

HISTORY AND BIOGRAPHY

Great men and women of Poland. Ed. by STEPHEN P. MIZWA. 425 p., 30 portraits, 15×24 cm. *Macmillan*, $4. This is the first attempt to present in a single volume and in English, biographical sketches of Polish men and women who have achieved fame, greatness and immortality. The 30 notable Poles who are represented were selected by a jury from a ranked list of 133 names submitted by leading Polish scholars and institutions. In the final list of 30, there are 8 men of letters and historians, 9 statesmen and soldiers, 5 painters, musicians and dramatic artists, 4 social reformers and educational and religious thinkers, and 4 scientists. Many of these are well known to contemporary Americans—Pituski, Kósciuszko, Pilsudski, Conrad, Paderewski, Chopin, Matejko, Modjeska, and the four scientists, Copernicus, Maria Curie, Wróblewski and Olszewski. Incidentally, the work of the last two men provides a beautiful example of the fertility of research engaged in cooperatively by a theorist and an experimentalist. The biography of COPERNICUS was written by an anonymous Polish scientist; that of MARIA CURIE, by MARY LANDON SAGUE, of Vassar College; and those of WRÓBLEWSKI and OLSZEWSKI, by TADEUSZ ESTREICHER, the Polish chemist. It is of interest that 5 additional scientists were among the first 60 in the list of 133 names originally submitted. One of these—the great theoretical physicist, SMOLUCHOWSKI—was finally eliminated because his contributions lie in a "highly specialized sphere understood only by specialists." In this country, WILLARD GIBBS has been in a similar category.

Benjamin Franklin's experiments—A new edition of Franklin's *Experiments and observations on electricity.* I. BERNARD COHEN, 481 p., 15×22 cm. *Harvard Univ. Press*, $4. FRANKLIN'S *Experiments and observations on electricity* passed through five editions in English, three in French, one in German and one in Italian; it was one of the most popular and influential scientific works of its period and, according to JOSEPH PRIESTLEY (1733–1804), "bid fair to be handed down to posterity as expressive of the true principles of electricity; just as the Newtonian philosophy is of the true system of nature in general." However, the book has long been difficult to obtain, and only fragments of it have been reprinted. MR. COHEN'S work, which is based on the last English edition (1774), restores many pages that do not appear in the latest edition of FRANKLIN'S works, prints in full for the first time and under their correct dates the most important of Franklin's letters on electricity, and gives for the first time the replies to FRANKLIN made by PETER COLLINSON, "the most important single person in Franklin's scientific career." In editing this material, MR. COHEN, a trained historian of the sciences, has made only minor alterations, such as the correction of obvious misprints. In a 161-page introduction, he provides chapters on Franklin and science, Electricity before Franklin, Franklin's work in electricity and The editions of Franklin's book. Much of this material is new, and it "reveals a side of Franklin that has long been obscured by his greater political reputation."

Portraits of famous physicists, with biographical accounts by Henry Crew. 12 portraits and biographies, 25×36 cm, *Scripta Mathematica* (Amsterdam Ave. and 186th St., New York) $3.75. In 1937 a committee representing the AMERICAN JOURNAL OF PHYSICS was formed for the purpose of planning a portfolio of portraits of eminent physicists, to be published by *Scripta Mathematica*. Although the issuance of the portfolio was unavoidably delayed several times, it has now appeared and has already been distributed to advance subscribers. Similar in form and of the same high quality as the portfolios of portraits of mathematicians and philosophers previously published in this series [Am. J. Phys. **4**, 216 (1936); **7**, 425 (1939); **9**, 251 (1941)], the present collection should prove to be interesting and useful to every teacher and student of physics, whether or not he has a special interest in the history of science. The portraits are loosely enclosed in attractive folders printed in two colors and containing interesting and forcefully written biographical sketches of the subjects by PROFESSOR CREW. The 12 eminent nonliving physicists represented are GALILEO, HUYGENS, NEWTON, AMPÈRE, FRESNEL, FARADAY, JOULE, CLAUSIUS, MAXWELL, GIBBS, HERTZ and ROWLAND. In making this selection the committee did not pretend to be able to determine the 12 most eminent physicists of all time but selected those who are considered to be genuinely great and whose lives and portraits are of deep interest to all students of the science. Some 50 names appeared on the initial list considered by the committee, and agreement was soon reached on ten names. The remaining two—HERTZ and ROWLAND—were selected from a group that also included ARCHIMEDES, BOLTZMANN, GILBERT, HELMHOLTZ, KELVIN, KIRCHHOFF and OERSTED. Other names strongly advocated by one or more members of the committee were ALHAZEN, BLACK, CARNOT, COULOMB, HENRY, LORENTZ, RAYLEIGH, ROENTGEN, RUMFORD, STEVIN, VOLTA and YOUNG. When the list was compiled, RUTHERFORD and J. J. THOMSON were still living. The committee for the portfolio consisted of R. T. Birge, Henry Crew, W. F. Magie, L. W. Taylor, E. C. Watson, J. Ginsburg and the editor of this journal.

SCIENCE TEACHING; EDUCATION

Modern high school curriculum. PAUL E. BELTING, Assistant State Superintendent of Public Instruction in Illinois, AND NATALIA MAREE BELTING, Department of History, University of Illinois. 276 p., 15×23 cm. *Garrard Press*, $2.50. The principles underlying the secondary school curriculum are outlined and then applied to 12 school

subjects, with particular reference to the Illinois schools. In the application, about 10 percent of the space is allotted to science and 5 percent to mathematics.

Enriched teaching of science in the high schools. Ed. 2. MAXIE NAVE WOODRING, Teachers College, Columbia University, MERVIN E. OAKES, Queens College AND H. EMMETT BROWN, Lincoln School, Teachers College, Columbia University. 413 p., 15×23 cm. *Bureau of Publications, Teachers College, Columbia Univ.*, $3.25. The 1928 edition of this useful handbook for teachers of the school sciences has been thoroughly revised and brought up to date. Part I contains classified and annotated lists of hundreds of free and low cost materials—pamphlets, exhibits, charts, samples, models, and so forth. Part II similarly lists a large number of materials and sources pertaining to such matters as scientific equipment and supplies, audio-visual technics, tests, science clubs, vocational guidance, professional societies, and books and periodicals for the science teacher.

POPULAR BOOKS

Atoms in action. Ed. 2. GEORGE RUSSELL HARRISON, Professor of Physics, Massachusetts Institute of Technology. 413 p., 19 illustrations, 15×22 cm. *Morrow*, $3.50. The original version of this remarkable book went through four printings and has either appeared or will soon appear in seven foreign editions in as many different languages. The present revised edition contains a new chapter on "Science in war and after," in which various military developments emerging in this "physicist's war" are described and some of the social implications of physical discoveries and inventions are discussed. We should like to repeat what we said earlier [Am. J. Phys. **8**, 138 (1940)], that this book demonstrates what can be accomplished when an able scientist of broad interests attacks the task of presenting his subject to the general public with the same determination and respect for the difficulties involved as he would have in dealing with a purely scientific problem.

MISCELLANEOUS BOOKS

The retina. S. L. POLYAK, Associate Professor of Anatomy, University of Chicago. 617 p., 100 fig., 20×27 cm. *Univ. of Chicago Press*, $10. The anatomy and histology of the human and the simian retina, including visual functions and the experimental technics for the investigation of the eye, the retina and the visual pathway, are treated in great detail in this elaborate work. Considerable space is also given to the history of physiological optics from earliest beginnings up to modern times. The bibliography alone occupies 54 pages. A second volume now in preparation will deal with the visual pathway.

The spectrochemical analysis of metals and alloys. F. TWYMAN, Managing Director, Adam Hilger, Ltd. 363 p., 61 illustrations, 22 tables, 14×22 cm. *Chemical Publishing Co.*, $8.50. Prepared expressly for the use of teachers and students of metallurgy and industrial metallurgists, this book deals authoritatively with the scope, nature and detailed technics of spectrochemical analysis as it is carried out in modern industrial laboratories. The author's adoption of the term *spectrochemical analysis* is most appropriate; for, as he points out, the older term "spectrum analysis" is ambiguous in that it may connote either analysis *by means of* the spectrum or an analysis *of* the spectrum.

The photochemistry of gases. WILLIAM ALBERT NOYES, JR., Professor of Chemistry, University of Rochester, AND PHILIP ALBERT LEIGHTON, Professor of Chemistry, Stanford University. 484 p., 66 fig., 37 tables, 15×23 cm. *Reinhold*, $10. Issued as one of the *American Chemical Society monograph series*, this book on photochemical reactions in the gas phase is planned to serve both as a textbook for a course on photochemistry and as a critical survey of the subject. The main topics treated are the experimental technics of photochemistry, a survey of spectroscopy, photochemical kinetics, and photochemical reactions resulting from absorption by atoms, by diatomic molecules and by polyatomic molecules. Many photochemical data and an extensive, classified bibliography are included.

Color and light in painting. ROLAND ROOD. Ed. by GEORGE L. STOUT, Fogg Museum of Art. 307 p., 34 fig., 15×23 cm. *Columbia Univ. Press*, $3.25. This book is of interest to physicists mainly because it is an example of how painting practice may be dealt with against a background of science and philosophy and because the author as a young man served as assistant to his father, OGDEN ROOD, then professor of physics in Columbia University. Since the father worked at art as an avocation and wrote a textbook on color which is well known among artists, "it is not surprising that the son should have turned to painting as a profession or that he should have carried into it an inquiring mind academically schooled and disciplined further in a physical laboratory." The present volume is based on manuscripts and notes for a book about color and painting upon which ROLAND ROOD had been working for some 15 years prior to his death, in 1927. This material was sorted and typed by his widow before her death in 1940 and was assembled for publication in its present form by GEORGE L. STOUT. Some of the main topics discussed in the book are a theory of beauty, sensation, production of color by addition and subtraction, broken color and luster, color contrasts, and attention and drawing. The author of course worked without benefit of the advances in the physics of color and in art that have been made in the last 15 years.

DIGEST OF PERIODICAL LITERATURE

Cleaning Fine Copper Wire

The end of a piece of litz or other fine copper wire may be cleaned for soldering by the following simple method. The wire is wrapped around a piece of heavy copper wire, of diameter, say, $\frac{1}{8}$ in. (B. & S. No. 8) and is then brought to a red heat in a flame and quenched in alcohol. The thermal capacity of the heavy wire prevents cooling of the fine wire before it is quenched. By this method both silk and enamel insulation are removed from fine wire, even from the part that is in contact with the heavy wire.—D. BELL, *J. Sci. Inst.* **19**, 79 (1942).—J. D. E.

A Demonstration Foucault Pendulum

The bob of the pendulum is a 75-lb ball of lead, 7 in. in diameter. The pattern and mold were made and the casting done in the school shop. The head of a heavy bolt is imbedded in the lead to serve as a support. A hole is drilled transversely through the bolt and is reamed to a taper. An axial hole, large enough to take the suspension wire, is drilled in the bolt to meet the tapered hole. The end of the wire passes through the axial hole and out through the large end of the taper. A tapered plug holds the end of the wire; any tendency to slipping draws the plug more firmly into the hole. The upper end of the wire is held in the same way in the end of a hook, whose hardened point rests on the upper surface of a hardened steel block supported from the ceiling of the room.

To make visible the rotation of the plane of vibration of the pendulum with respect to the earth, a light metal strip whose ends are bent up is clamped horizontally to the suspension wire and its shadow is cast on a screen by a projection lantern. The rotation is made evident in a few minutes by the separation of the shadows of the ends of the strip.—M. J. W. PHILLIPS, *Sch. Sci. and Math.* **42**, 413–418 (1942).—J. D. E.

A Challenge to Physicists

From a long-range point of view the deepest issues of the present crisis are intellectual, and our failure so far has been an intellectual failure—intellectual sloth, lack of imagination, and wishful thinking. The race will not save itself until it achieves intellectual morale, the two chief components of which are perhaps intellectual integrity and a fierce conviction that man *can* become the master of his fate. But it is just these components that physicists are peculiarly likely to possess; and if they will only make others see their own wider vision, their ultimate influence will far transcend that of any possible technologic contribution.—P. W. BRIDGMAN, *J. App. Phys.* **13**, 209 (1942).—D. R.

The Decline of the Textbook in College Courses

Teachers should think twice before they abandon the single textbook—owned, read and studied by the student. Such a book may well be supplemented by lists of references; but the plan of providing students with nothing but a long list of books generally undermines the validity of group teaching. The injunction to read widely and at random digs many a pitfall for the student. Libraries are magnificent institutions to enrich knowledge; but as a source of simple, basic and fundamental information they do not work well. In a field in which students are learning fundamentals, the practical situation demands the use of a basic textbook away from the library; only thus can a group actually secure a common core of reading experiences which they may use to supplement one another's learnings by wide reading and group discussion.—A. G. MELVIN, *Sch. and Soc.* **55**, 558–560 (1942).—D. R.

Some English Views on Science Teaching

All sound science teaching rests on several things, but above all on careful and accurate observation. Such observation is not within the capacity of an untrained observer; the students have to be taught to observe. Critical observation breeds a critical habit of mind and a capacity for using the information obtained by the observation. It provokes and promotes thought. Training in observation and in the habit of mind it engenders is one great contribution that science has to make to education.

To train students in accurate observation is a severe test of the teacher's ability. Many students, rightly and naturally, take much pleasure in making preparations, in analysis, in dissection and in compiling books of drawings, but these things by themselves are not science. They are but means to the end, which is training in the art of close and accurate observation. There is a value also in the confidence the student gets from his ability to do something in a workman-like manner. Sound work in the sciences acts as a protection against over-hasty generalizations. The diversity of natural phenomena stimulates curiosity and keeps the mind alert. It makes the observer watchful, and it encourages a habit of mind that demands evidence rather than unsupported statements.

Probably the greatest hindrance to science teaching is the examination system. Rightly or wrongly, teachers may consider it their main task to get their students through examinations, and if they do not themselves think so, that point of view may be imposed on them either by the administration or by public opinion working through the administration. The objective becomes the piling up of examination successes, and the bigger and more impressive the pile, the better the work of the school and of the teacher is supposed to be. All this reacts on the mentality of the students, who get more and more the idea that the aim of their schooling is to enable them to pass examinations; the more stupid and a very few with an abnormal dose of common sense may escape this. To pass the more showy examinations, specialization is thought to be needed, and this thought is gaining ground to such an extent that there is even talk of introducing

research into the schools, a charming illustration of a tendency not unknown in educational schemes, that of starting a building with the roof and working downwards. —Editorial, *Nature* **149**, 447–449 (1942).—D. R.

Physics Enrolments in Secondary Schools

At intervals during the past 30 years, studies have been made of the sequence of science courses and enrolments in the various courses in the secondary schools of the United States. In 1909 a questionary to which 276 replies were received indicated that physics courses were offered in the eleventh grade in most schools. In 1923 a new questionary elicited replies from 368 schools. It was found that more students were taking biology, chemistry and physics than 15 years before. Physics and chemistry were about equally divided between the eleventh and twelfth years.

The junior high school was coming into prominence during the twenties, and in 1930 a broader questionary was sent out to both junior and senior high schools. Replies were received from 528 schools. Physics showed a tendency to move to the twelfth year, and it was the preferred college preparatory science. Slightly more than half these schools reported increased enrolments in physics.

Because of the rise of the "progressive" point of view during the past decade a new and greatly enlarged questionary was sent in 1941 to 2200 junior and senior high schools; 655 replies were received, from every state in the Union. Physics was found to be offered in the twelfth grade in 56.20 percent of the schools answering and in the eleventh grade in 41.87 percent. It is to be noted that in many southern states there are only 11 grades in all. On the basis of the replies received, it appears that physics is taken by 5.6 percent of the total enrolment (Fig. 1) and

FIG. 1. Distribution of physics by areas, based on total school enrolments.

by 7.95 percent of the science enrolment in the United States. Physics comprises 28 percent of the science offering in the eleventh grade and 37.20 percent of that in the twelfth grade—a steady loss since 1923. The figures indicate that physics is losing ground; but there are many new kinds of physical science being offered. Students who are not preparing for college are frequently taken care of by such courses as "senior science," "survey science," "descriptive chemistry and physics," and "general chemistry and physics."—G. W. HUNTER AND L. SPORE, *Sci. Ed.* **25**, 359–370 (1941); **26**, 66–77 (1942).—J. D. E.

History of the British Thermal Unit

According to the *Oxford Dictionary*, the first use of the term *British thermal unit* occurs in the "Catalogue of the Special Loan Collection of Scientific Apparatus at the South Kensington Museum, 1876," in the description of Hargreaves's thermoradiometer. From the context it appears that the use of the qualifying adjective *British* was merely to identify the unit as that used by Joule, Thomson, Rankine and others, as distinguished from the calory. Apparently by 1895 the unit had been defined explicitly, for in that year, in the tenth edition of Jamieson's *Textbook of steam and steam engines*, it is described as "the quantity of heat required to raise 1 lb of water by 1°F when at its maximum density, i.e., from 39.1° to 40.1°F." In the same year Callendar used the unit, calling it a "pound degree Fahrenheit."

There still appears to be uncertainty as to the temperature at which the rise of 1°F is to be measured, since there seems to be no legal definition of it. However, the British Standards Institution (1926 and 1936) defines the British thermal unit as the quantity of heat required to raise the temperature of 1 lb of water from 60° to 61°F.—R. W. Powell, *Nature* **149**, 525–526 (1942).—J.D.E.

Check List of Periodical Literature

Role of photography in the detection and measurement of radiation. E. R. DAVIES, *Nature* **149**, 430–432 (1942). A brief survey.

The nature of static friction. W. CLAYPOOLE AND D. B. COOK. *J. Frank. Inst.* **233**, 453–463 (1942).

Solar radiation and the state of the atmosphere. H. T. STETSON, *Sci. Mo.* **54**, 513–528 (1942).

Geological applications of nuclear physics. C. GOODMAN, *J. App. Phys.* **13**, 276–289 (1942). How nuclear physics has and can contribute to an understanding of the history, the structure and the composition of the earth.

Explosives and their military applications. R. H. KENT, *J. App. Phys.* **13**, 348–354 (1942).

Magnetism and molecular structure. P. W. SELWOOD, *J. Chem. Ed.* **19**, 181–188 (1942). A discussion of methods of measuring magnetic susceptibilities, with applications to the determination of molecular structure.

Electrical properties of solids. R. M. FUOSS, *J. Chem. Ed.* **19**, 190–193 and 231–235 (1942). A survey article.

A method of teaching thermodynamic functions. H. BECHER. *J. Chem. Ed.* **19**, 237–238 (1942). Simple rectangular diagrams are used to show the relationships among the various thermodynamic quantities.